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VOL. XLIX. JANUARY—JUNE, 1864.

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THE

RETROSPECT OF MEDICINE:

BEING

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AND

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A SYNOPSIS,

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AFFECTIONS OF THE SYSTEM GENERALLY.

Enteric Fever.—Foul air merely favours its invasion; but its true cause is something much more specific, some ferment which is peculiar to the disease, and has hitherto eluded our search. We shall do well to encourage better drains, more and better water-closets, and better ventilation of dwellings; still we must not count upon thus extirpating enteric fever. (Dr. R. Christison, p. 5.)

Pain.—Pain is a disease *per se*. It occurs alone, unaccompanied by the other three symptoms of inflammation, redness, heat, and swelling. It results in most cases from an abnormal state of the sensory nerve fibres, and this arises, and is inseparable from, a state of constitutional debility. Pain in one fixed spot in the spine, increased on pressure, does not always indicate spinal disease; many such pains yield at once to the local employment of opium in extract, and to the internal administration of bark, valerian, and ammonia. The pain is far more severe than in cases of real spinal disease. (Mr. F. C. Skey, p. 335.)

Rheumatic Pains in the Tendons of the Hamstring Muscles.—The constant local application of sulphur is often of much use in these troublesome cases. It may be dusted inside the stockings, or inside a flannel bandage. The skin becomes hotter, more excited, and disposed to sweating. (Dr. Renard, p. 19.)

Scarlet Fever.—*Chlorine.*—The throat affection of scarlet fever must be considered to be an effort at elimination of the poison from the blood. Hence, a great point in the treatment is to give some remedy frequently which, in passing over the throat, shall neutralize the poison which nature is casting off, and so prevent it being swallowed over and over again, and the system being more and more loaded. A strong solution of chlorine thould be given night and day, every

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by the presence of irritating secretions in the primæ viæ. Such patients may never have suffered from gout or rheumatism in their lives. The pain is, moreover, usually felt down the course of both sciatic nerves, instead of being confined to one side, the tongue is furred, and the bowels are costive. Ordinary aperients do not remove old accumulations from the bowels. The best purgative for this purpose is a powder composed of equal parts of guaiacum and sulphur, of which four scruples must be taken three times a day. (Dr. H. W. Fuller, p. 388.)

TETANUS.—*Local Application of Tobacco.*—In a case of tetanus from burn, which occurred to Mr. Tyrrell, of Dublin, immediately on the supervention of the symptoms the burn was covered with lint saturated in an infusion of tobacco. Wine and beef-tea were freely administered. The treatment seemed to be of much use, and the case ended successfully. (Mr. Tyrrell, p. 58.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ACUPRESSURE *versus* DELIGATION OF ARTERIES.—There can be no doubt that the use of the ligature tends to prevent primary union in cases of amputation, and Dr. Simpson, the inventor of the method of closing vessels known as acupressure, and those gentlemen who have given this plan a fair trial, have become more and more convinced of its utility. The needle need not be retained even over a very large artery, as the femoral, for a very long period; in one case four days, in another two only elapsed before its removal. To procure union by the first intention other precautions are necessary, otherwise our object in employing acupressure is not gained. The sides or flaps of the wound must never require pulling or tension to keep them in apposition, and wounds or flaps must be more systematically shaped than they are even at present. The dead particles of bone-dust must be carefully removed from the surfaces of flaps by a stream of water, or they otherwise act as dead foreign bodies. Coagula of blood invariably act as dead foreign bodies; to prevent their formation the wound must not be closed until all bleeding has ceased. The flaps must be carefully closed from below upwards, and never the edges first, otherwise atmospheric air is included. The mere shallow cutaneous edges of flaps are usually brought together by superficial stitches; instead of this metallic sutures should be planted so deeply as to embrace to a considerable extent the sides of the wound. It is better to avoid much strapping after amputations. (Dr. J. Y. Simpson, p. 105.)

Methods of applying Acupressure.—There are three methods of arresting hemorrhage from a vessel by means of acupressure:—

1. Pass a long needle twice through the flaps or sides of a wound so as to cross over and compress the bleeding artery. The only portion of the needle which is left exposed internally, on the fresh surface of the wound, is the small middle portion of it. The head and point are exposed externally.

2. Thread a common short sewing needle with iron wire, and dip it down into the soft textures a little to one side of the vessel, then raise it and bridge it over the artery, then dip it down again and thrust it into the soft tissues on the other side of the vessel. Care must be taken to press the end of the needle down upon the mouth or tube of the bleeding artery with force sufficient to arrest the hemorrhage.

3. Let the point of the needle be entered a few lines to one side of the vessel (on the surface of the flap), then under the vessel, and let it emerge a few lines beyond the vessel. Now throw a noose of wire over the point of the needle, carry it across the mouth or site of the vessel, and, passing it around the eye of the needle, draw it sufficiently tight to close the vessel, when it may be fixed by a twist or half twist around the stem of the needle. The needle may be withdrawn at the proper time by simply pulling at the wire with which it is threaded, and the noose, being liberated, can be withdrawn also. (Dr. J. Y. Simpson, p. 109.)

Does Secondary Hemorrhage frequently result from the employment of Acupressure?—In eleven cases in which the ligature was used at the Carlisle Hospital, there were four cases of secondary hemorrhage; in eleven in which acupressure was employed there was only one such case. It is necessary when we employ acupressure to remember this, that so long as the pulsation can be felt going on along the artery, down to the point of compression, it is unsafe to remove the needle, as permanent occlusion has not yet taken place. (Dr. J. Y. Simpson, p. 116.)

ANEURISM.—*Application of Indices to Pressure Instruments.*—The bad consequences resulting from, and the ill success so often attending the application of pressure for the cure of aneurismal tumours, arises from its defective application. Too frequently, excessive pressure is employed. This, it is probable, may be obviated by the application of indices to the instruments. By this means the minimum force by which the circulation can be arrested is ascertainable, and when the pad is subsequently reapplied by assistants there are less likely to be errors in the direction of pressure and in its amount. The amount of pressure should be registered by means of a needle on a scale, communicating with a strong spring. (Mr. E. Hart, p. 127.)

Incurable Aneurism.—New Method of Treating.—In a case of sacculated aneurism of the ascending aorta a small-pointed canula was inserted into the tumour, and a large quantity of of fine iron wire was passed into the interior of the sac. The operation lasted an hour, and the quantity introduced was twenty-six yards. No pain or inconvenience was caused. The pulse was at once reduced in frequency, and pulsation almost completely ceased in the tumour, which diminished in size. In a few days, however, rigors set in and the patient died with symptoms of pyæmia. The idea is one of value, and treatment of this nature with the view of producing consolidation of the tumour may be occasionally adopted in other aneurisms than those of the aorta, with less risk and more success. (Mr. C. H. Moore, Dr. Murchison, p. 129.)

SUBCUTANEOUS NÆVI.—*Removal of by Ligature without loss of Skin.*—Nævi which are entirely subcutaneous may be removed without any loss of skin, by passing the ligatures round the tumour, between it and the skin, so that the tumour only is included. The first step in the operation is to transfix the base of the tumour in its longest diameter with a needle carrying a double thread of strong silk ligature; the thread must then be cut close to the needle, and the two threads carried, by means of a slightly bent eyed silver probe, round the halves of the tumour subcutaneously, and tied. When the tumour is large the plan illustrated by the woodcuts at pp. 121-2 must be followed. (Mr. J. Jardine Murray, p. 120.)

VARICOSE VEINS.—The plan of attempting to obliterate varicose veins by means of a needle or pin beneath them, and a ligature, is not only frequently unsuccessful, but is actually in many cases productive of harm. A far better plan is to apply to the most projecting parts of the veins a paste composed of two-fifths pure potash and three-fifths lime, mixed up with a little alcohol. It should be applied through a series of about four layers of adhesive plaster, having a hole through them about the size of a three penny piece, or smaller. The entire vein becomes obliterated, and is undetectable by the finger on pressure. As many as from ten to twenty-five small eschars may be applied between the ankle and the knee. (Mr. F. C. Skey, p. 126.)

VENOUS HEMORRHAGE AFTER AMPUTATION.—It occasionally happens that after amputation of the thigh there is very troublesome hemorrhage from the femoral vein. In such a case, grasp the mouth of the vein by a very small pair of

Dieffenbach's artery forceps, and leave it in the interior of the wound, but having a string attached. Its pressure effectually controls the flow of blood; its bulk forms no obstacle to bringing the parts into close approximation; and it can be removed with the greatest ease, almost indeed with the same facility that we draw away a ligature. It is never safe to tie a vein. (Mr. G. H. Porter, p. 124.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ASTHMA.—*Iodide of Potassium.*—There are many cases of asthma in which iodide of potassium acts quite as a specific. When there are other indications for its use, as thick urine, gouty or rheumatic tendency, it is well worth employing. It does not usually relieve until it has been taken some days. (Dr. H. Salter, p. 71.)

CHRONIC BRONCHITIS.—*Larch Bark.*—In cases of chronic bronchitis in which expectorants have ceased to be useful, and the profuse expectoration is rather to be checked than encouraged, a remedy the use of which is attended with marked success is the tincture of larch bark. It should be given in doses of twenty to thirty minims, in a mixture consisting of tincture of gentian, nitro-muriatic acid, and water, with or without ipecacuanha wine or tincture of camphor. It is a remedy which never disagrees with the stomach. The employment of this drug is of course contraindicated in cases of acute bronchitis. (Dr. H. Greenhow, p. 75.)

HOOPING COUGH.—*Bromide of Ammonium.*—This remedy has been extensively tried lately in hooping cough, and it has been found to have a decided effect in arresting the whoop. The anæsthetic effect of the remedy on the nerves of the throat claimed for it by Dr. Gibb, would lead us to expect benefit from it in these troublesome cases. A grain may be given for each year of age three times a day. All cases of hooping cough are not equally benefitted by the remedy; some pertinaciously resist its action, while others readily yield to it. (Dr. G. Harley, p. 76.)

LARYNGISMUS STRIDULUS.—In cases of this troublesome affection connected with dentition a bag of ice applied to the cervico-dorsal portion of the spine will frequently arrest the attacks. (Dr. Ramskill, p. 63.)

TRACHEOTOMY.—*Excision of a Portion of the Trachea.*—It is of great importance in tracheotomy that a portion of the tube should be excised. The removal of a thin elliptical piece about half an inch in length is sufficient. The canula can [illegible] and cough. (Mr. G. H. Porter, p. 14.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

DIPHTHERIA.—*Local Applications to the Throat.*—Hydrochloric acid, diluted with an equal quantity of water, is more efficacious, perhaps, than nitrate of silver. Whichever is used, its application should be limited to the exudation, and the parts simply inflamed avoided as much as possible. Gargles are never of much service. (Dr. W. Gibson, p. 79.)

It is a plan often of considerable service to dust the throat with powdered alum, or tannin, or a mixture of both, which may be blown in through a quill. (Dr. W. Gibson, p. 79.)

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I believe that, as a rule, anthrax need not be be cut at all. (In some cases, however, the use of the knife *is* necessary.) As soon as the parts get support the extension of sub-fascial exudation ceases. This support must be given round the margin of the inflamed part, by means of spiral strapping with adhesive plaister. The efforts of the matter to get vent are then directed to the surface and the centre, and the necessary sloughing and ulcerative processes are more rapidly performed. (Mr. M. H. Collis, p. 183.)

Carbuncle and Sloughing Sores.—Carbolic acid is one of the best caustic applications we have. It is a strong caustic when undiluted, as well as antiseptic. (Mr. Turner, p. 372.)

BURNS.—Mr. Skey treats his cases of burn by the free application of a solution of nitrate of silver, ten grains to the ounce. The burnt surface is then covered with cotton wool. The pain is increased at first, but it rapidly subsides. This is not applicable where there is much destruction of tissue. (Mr. F. C. Skey, p. 178.)

Severe Burns.—Internal Administration of Belladonna.—As belladonna diminishes that state of the nervous functions under which reflex inflammations are likely to originate, it is a remedy which ought to be administered in cases of severe burns, for death from this cause, in the majority of cases, arises from ulcers of the intestine, pneumonia, &c.,

which may be called reflex inflammations. Opium is contra-indicated. (Mr. Hutchinson, p. 180.)

CHRONIC ULCERS.—*Opium.*—The more chronic the ulcer, the larger its size, the more aged the subject, the more remarkable is the influence of opium in effecting its cure. A dose of ten or fifteen drops of tincture of opium night and morning, will in five or six days cause a pale flat bloodless ulcer to exhibit a number of minute red points, which daily increasing in number rise up and form healthy granulations. Cases of twenty years' duration are frequently easily cured by this simple means. Opium judiciously employed has a most salutory effect on the constitution. (Mr. F. C. Skey, p. 189.)

ENLARGED BURSÆ.—When a bursa, as that on the knee, is enlarged, the treatment usually adopted is blistering. This is quite ineffectual. The most successful plan is to pass a moderately strong silk thread through it. In a period varying from three to ten or twelve days it is converted into an ordinary abscess. The thread may then be withdrawn, and the abscess treated like any other. The bursa is for ever obliterated. (Mr. F. C. Skey, p. 187.)

FAVUS.—*Carbolic Acid.*—A solution of carbolic acid in glycerine (in the proportion of one part of the acid to twenty-five of glycerine) may be applied with satisfactory results. The crusts must first be removed. (Dr. J. Watson, p. 377.)

GANGLIONS ON THE BACK OF THE WRIST.—These are usually ruptured by a smart blow from the back of a book, or are pricked by means of a lancet, and emptied of their contents. These plans are successful if the contents are *completely* evacuated. If any portion, however small, is left behind, the case is not cured. A thick pad of lint must be placed over the emptied cyst and a bandage tightly applied, that the opposite sides may unite, which they will usually do in forty-eight hours. (Mr. F. C. Skey, p. 188.)

GANGRENOUS AND FŒTID SORES.—Use carbolic acid as a lotion. Freely diluted with water its caustic properties are lost, but its antiseptic and astringent powers still remain. (Dr. J. Watson, p. 373.)

LUPUS.—*Carbolic Acid.*—Carbolic acid dissolved in glycerine is one of the best applications we have for lupus. It may also be dissolved in acetic acid. (Mr. C. de Morgan, p. 372.)

PARASITIC AFFECTIONS OF THE SKIN.—In the five parasitic affections of the skin (Favus, Trichosis, Mentagra, Alopecia areata, and Chloasma or Pityriasis versicolor,) depilation is wholly unnecessary, except in mentagra (sycosis). The local treatment consists in removing crusts by thorough washing and combing of the hair, repeated daily, and when this is accomplished, in rubbing into the entire scalp, but most into the diseased patches, an ointment consisting of one part of the nitric oxide of mercury ointment, diluted with three parts of fresh lard. But constitutional treatment is also necessary. In favus, a disease of the scrofulous diathesis, cod-liver oil and iodide of iron must be administered. In some cases arsenic may be administered "with the most brilliant success." In chloasma (which occurs on the body) the skin may be sponged with a spirituous solution of bichloride of mercury in almond emulsion. (Mr. E. Wilson, p. 168.)

PITYRIASIS.—One form of pityriasis, Pityriasis versicolor, depends upon the presence of a parasite, and is curable only by its destruction. This may be readily accomplished by the sulphur vapour-bath, and the use of a weak nitric acid lotion during the day. When pityriasis occurs on the head, the hair should be cut moderately short, and the head washed once or twice a week with the yolk of egg and warm water, and every night the following liniment be well rubbed into the roots of the hair:—nitric oxide of mercury, ten grains; glycerine, half an ounce; cerate, one ounce; or ammonio-chloride of mercury, ten grains; cerate, one ounce. Tonics, as iron, quinine, mineral acids, or arsenic are frequently administered with advantage. (Mr. G. Nayler, p. 170.)

SMALL POX.—*Sarracenia Purpurea.*—The decoction requires to be carefully prepared; the root should be thinly sliced, and boiled in a closely-covered vessel; and for adults the quantity of the root should be increased to three or four ounces to the three pints of water, and about eight ounces administered in the twenty-four hours, in four doses. Its action in small-pox is unmistakeable. (Dr. J. Taylor, p. 31.)

TINEA.—The head having been thoroughly cleansed, the crusts removed, and the hair cut close to the scalp, paint the surface over with the oleum picis juniperi (oil of cade). The next day wash the head with soap and commence depilation. When a small extent of surface has been freed from hairs, brush it over with a solution of the bichloride of mercury to destroy the fungus. Four or five hours after the operation anoint the head thoroughly with a parasiticidal ointment, containing the yellow subsulphate of mercury, in the proportion of about half a drachm to each two ounces of lard. As the depilation cannot be completed at one sitting it must be proceeded with daily until complete. (M. Bazin, p. 170.)

[It is probable that the employment of the oil of cade may be omitted, as M. Hardy, another eminent authority on this subject, does not employ it.]

AFFECTIONS OF THE EYE AND EAR.

Atropine.—*Gelatine Atropine Paper.*—The trouble of removing the little squares of paper, imbued with atropine, is sometimes considerable, and may be avoided by employing gelatine rolled into very thin sheets instead. This must, of course, be divided into little squares, each containing the proper amount of atropine. It becomes completely and rapidly dissolved; and after its introduction no further trouble is required. (Mr. Streatfeild, p. 201.)

Amount of Atropine Necessary to Dilate the Pupil.—The quantity of atropine generally used for the purpose of dilating the pupil is far too great, having the effect of paralysing completely the accommodation of the eye, with consequent disturbance of vision for five, six, or more days. The square of $\frac{1}{8}$th of an inch of atropine paper, as used by Mr. Streatfeild, contains $\frac{1}{240}$ of a grain of sulphate of atropia; $\frac{1}{100000}$ of a grain, however, very effectually produces dilatation, which lasts nearly twenty-four hours. The accommodation is very slightly affected by this amount, and recontraction may be readily produced, if desired, by Calabar bean paper, which has not hitherto been prepared sufficiently strong to counteract the action of a larger quantity of atropine. Both kinds of paper may be obtained from Messrs. Savory and Moore. (Mr. E. Hart, pp. 202, 210.)

Calabar Bean.—The Calabar bean has the most energetic effect in causing contraction of the pupil; it acts painlessly, quickly, and without any irritation. Besides cases of prolapse of the iris through a corneal wound from accident, it will, probably, prove valuable in those annoying cases where after extraction of the lens the iris prolapses, and will not remain out of the lips of the corneal wound. The spirit extract, dissolved in glycerine, is an excellent and convenient preparation; but the prepared paper, saturated with a tincture of the bean, and then dried like the atropine paper, suggested by Mr. Streatfeild, is the most portable form of preparation, and will be, consequently, most commonly used. The tincture of the bean requires dilution with about ten parts of water, lest the spirit prove injurious. (Mr. T. Nunneley, p. 205.)

Chronic Catarrh of the Auditory Meatus and Tympanum.—In cases of chronic catarrhal inflammation of the meatus and tympanum, when there is a red and granular condition of the membrana tympani, with or without minute polypoid growths, no treatment is better than the introduction of a little powdered alum. It may be blown in through a piece of india-rubber tubing, or introduced on a camel's hair-brush. (Mr. Hinton, p. 250.)

CILIARY BLEPHARITIS.—After removing the scales which cover the margin of the lid, apply tincture of iodine to the affected parts. It should be diluted with an equal quantity of distilled water, and applied with a camel's hair-brush. (Dr. Fano, p. 204.)

"CUPPING" OF THE OPTIC DISC.—"Cupping" of the optic disc is supposed to be a positive evidence of pressure on the retina. It is usually seen in cases of glaucoma. This cupping is, however, sometimes seen apart from glaucoma, and even where the eyeball is not hard. The usual explanation of its mode of production cannot, therefore, be correct. (Mr. H. Walton, p. 218.)

DISPLACEMENT OF THE PUNCTA.—The puncta ought to face inwards upon the surface of the globe, otherwise they do not drain its surface of the superfluous amount of tears. Should they be everted, or pushed away by an enlargement of the caruncle, this may be removed, or the canal may be laid open, and so converted into an open drain. Where the punctum and canal are of normal size, this operation is most easily done by means of a pair of scissors especially adapted for the purpose. If the puncta are contracted, a very fine director, or a fine probe bistoury is more convenient than scissors. It is important not to wound the mucous lining of the canal, except at the part where it is laid open. The passage of a fine probe every two or three days will prevent reunion of the divided edges of the canal. (Mr. G. Critchett, p. 191.)

FISTULA LACHRYMALIS.—The first thing is to restore permeability of the nasal duct in the usual way, and then to close the fistulous aperture by paring the edges of the skin, and bringing them together with fine sutures, so as, if possible, to obtain union by the first intention. (Mr. G. Critchett, p. 200.)

INFLAMMATION OF THE TYMPANUM IN CASES OF DEAFNESS.—Place a piece of pretty firm elastic tubing in one nostril of the patient, and whilst he is in the act of swallowing, the other nostril being firmly closed, let the surgeon blow air forcibly in. This is founded on Mr. Toynbee's discovery, that the eustachian tube is naturally closed, but is opened by the tensor and levator palati muscles during the act of deglutition. The entrance of air into the tympanum, when the plan recommended is tried, may be heard by means of the ordinary otoscope. (Mr. Hinton, p. 250.)

IRIDECTOMY IN GLAUCOMA.—In the onset of an attack of acute glaucoma, when there is pain and increased tension of the eye-ball, it is unnecessary to resort to the severe operation of iridectomy. The division of the ciliary muscle, as recommended by Mr. Hancock, generally relieves the more urgent symptoms, that the after-treatment becomes a matter of little or no anxiety as to the ultimate condition of vision. But in the more advanced stages of the disorder, iridectomy should be at once resorted to. (Mr. J. Hogg, p. 237.)

The operation of iridectomy is by no means universally adopted by the profession as the best and only means of cure for glaucoma. But it is admitted by its opponents, that by all ordinary treatment the disease is quite incurable. Glaucoma is a term applied to, probably, several morbid processes, by which vision is destroyed; all, however, having this, in common, that they destroy the natural elasticity of the globe. Iridectomy at once restores this elasticity, and any sight existing at the time of the operation is preserved. Moreover, the restoration of elasticity to the eye-ball gives to the partially paralysed retina the best chance of recovering some of its functions and sensitiveness. (Mr. G. Critchett, p. 211.)

The benefit derived from iridectomy is not from any direct effect on the iris, but solely in consequence of the evacuation of the aqueous humour. It is not the iridectomy which does this, but it takes place as an inevitable step of the operation. The evacuation of the aqueous humour is of the greatest use, not only in glaucoma, but in all inflammations of the eye-ball; for in all, there is over-distension of the globe, and by no other means can this over-distension be so effectually relieved. It may be, and generally is, necessary to repeat the tapping more than once; but no harm accrues from a repetition of this mild operation, and this can scarcely be said of an operation in which a portion of the iris is removed. This proceeding, must, however, only be considered as an aid, and to be used along with other means. (Mr. H. Walton, p. 213.)

The indiscriminate use of iridectomy is fraught with danger. It is far from a slight operation, and serious evils not unfrequently attend, or follow its performance. Many of the cases for which it is recommended are amenable to general treatment. (Mr. W. Cooper, p. 221.)

OBSTRUCTION OF THE EUSTACHIAN TUBES.—*Politzer's Treatment.*—To ascertain whether an eustachian tube is pervious or not, let the patient swallow a little saliva, the mouth and nose being closed; if it is pervious, the surgeon will hear most distinctly a faint crackling sound, produced apparently by a slight movement of the membrana tympani. This can be heard by means of the stethoscope placed upon the patient's ear, or by means of the otoscope, an elastic tube about eighteen inches in length, and ivory tipped at both ends for introduction into the external meati of surgeon and patient. In old standing cases of obstruction, where no treatment directed to existing inflammation is required, mechanical treatment can alone be of any use. Eustachian catheterism, or forcibly blowing the nose, at the same time holding the nose tightly so that no air can escape, are sometimes alone of use. Politzer's instrument consists of a piece of catheter open at the end and several inches long, to which is attached a pear-shaped india-rubber bottle. During the act of swallowing, the tube being inserted into one nostril which is tightly closed over it, the air is expelled forcibly from the ball. This must be repeated daily. (Dr. M'Call Anderson, p. 242.)

OBSTRUCTION OF THE NASAL DUCT.—The first step in the operation of catheterizing the nasal duct is the slitting up of the canaliculus. This is done best by means of a very fine pair of scissors, with the points slightly rounded. It may also be done by means of a very fine probe-pointed bistoury, or by a fine director and a sharp triangular knife. The operator should be provided with a set of probes graduated of different sizes. The bulbed probes recommended by Mr. Pridgin Teale, of Leeds, are more readily passed, and less likely to lacerate the mucous lining than those ordinarily employed. A moderate sized probe may be commenced with, and must be passed carefully into the sac, and then down the nasal duct. This requires great care and management. It may be left in a short time, and the operation must be repeated every three or four days. (Mr. G. Critchett, p. 195.)

PROLAPSE OF THE IRIS.—The use of Calabar bean has proved of singular benefit in many cases of prolapsed iris. In one very marked case which occurred recently at St. Mary's Hospital, it saved from excision a large portion of the iris. Fully a fourth of the iris had prolapsed through the corneal wound. A small square of the Calabar bean paper was introduced beneath the lid, and firm contraction of the pupil produced. The lid was closed, and a pad strapped over it. The wound healed, and the iris was saved. (Mr. E. Hart, p. 209.

Stricture of the Canaliculi.—The situation of the stricture is generally where the little mucous canal opens into the sac. This can be ascertained by passing a fine bulb probe in the direction of the canal towards the sac, that is, inwards and slightly upwards. If the probe has entered the sac, it is brought at once against the firm unyielding inner wall; whereas, if a point of stricture be reached, the resistance felt is elastic, and, as the probe is pushed on, the parts around it may be observed to move with it. If there is stricture of the lower canal only, lay open the upper one, and it will suffice to carry off the tears. If there is stricture of both canals, and there is much inconvenience resulting, pass a broad lancet-pointed needle, guarded, along the canal as far as it can be carried; then, as soon as the point of stricture is reached, the sheath must be drawn back, and the point pushed forward into the sac, its direction may then be changed so as to pass it perpendicularly down the nasal duct. A probe must then be passed down the nasal duct. (Mr. G. Critchett, p. 193.)

Small bougies, made of the dried stem of the sea tangle, may be employed with advantage. They are quite smooth, firm, and elastic, and may be passed very readily. When exposed to moisture, they expand to at least double, or even treble the former size. This material was first introduced by Dr. Sloan, of Ayr. (Mr. G. Critchett, p. 198.).

MIDWIFERY, AND THE DISEASES OF WOMEN, ETC.

Acupressure in Ovariotomy.—Instead of using ligatures which act more or less as foreign bodies—or the clamp which frequently causes dangerous dragging on the pedicle, all hemorrhage may be arrested by acupressure, and danger from both these sources avoided. After removing the tumour pass a glass headed needle two and a half inches long, twice through the ovarian stalk, at a point a short distance from the cut extremity. A portion of tissue must be left out on either side to which no compression is applied, that it may supply with blood and vitality the strangulated stump of the ovarian stalk. To compress the stalk sufficiently use a loop of strong tinned iron wire, twisting it half round the headed end of the needle, and then drawing it with sufficient tightness to prevent any discharge of blood from the inclosed vessels. A slender cylindrical iron hood, three-quarters of an inch in length must be placed over the sharp end of the needle to prevent injury from it, and this hood must be fixed to the head end of the acupressure needle by a duplicature of slender silver wire. The whole may now be allowed to return into the abdomen, the needle being carefully placed across the brim of the pelvis, but having a string attached to the headed end and left hanging out of the wound. In about three days or less the needle may be pulled away by means of the string, and the small hood will come with it. (Prof. J. Y. Simpson, p. 304.)

CERVIX UTERI.—*New Instrument for Dilating.*—Dr. Priestley recommends an instrument similar in principle to one sometimes used for stricture of the urethra, invented by Mr. Henry Thompson. It is somewhat similar to a uterine sound but composed of two lateral halves. These halves are separable at the portion corresponding to the cervix uteri, by means of a screw in the handle. (Dr. W. O. Priestley, p. 306, Mr. R. Ellis, p. 309.)

FIBROUS TUMOURS OF THE UTERUS.—Two years ago, Mr. Baker Brown showed that it is quite unnecessary to enucleate and remove fibrous tumours of the uterus, as they may be destroyed by gouging out a portion of their tissue. He recommended preliminary to this, incision of the os and cervix. He now finds that this incision will alone not only arrest the hemorrhage but the growth of the tumour. In some cases it decreases in size, and where small it will entirely disappear, more especially if of recent origin; and even where gouging is required, a much slighter operation is sufficient. Mr. Brown now never uses "Harper's instrument" but only a pair of long handled blunt-pointed curved scissors. It is very important that the incisions and whole vagina should be carefully and thoroughly plugged after the operation, with oiled lint, to prevent exposure to the air and consequent hysteritis. These tumours appear to be of very low vitality, and the slightest interference with them stops their growth. (Mr. I. B. Brown, p. 316.)

HAIR-PIN IN THE FEMALE BLADDER.—An interesting case is recorded by Mr. Porter of Dublin, in which a hair-pin was removed from the female bladder. He accomplished the dilatation of the urethra by the successive introduction of tents of prepared sponge each of larger size than the preceeding one. Having passed the index finger of his left hand into the bladder, he seized one prong of the pin with a pair of fine forceps, and having fixed the other prong against his finger he withdrew the pin easily. (Mr. G. H. Porter, p. 333.)

INVERTED UTERUS.—Supposing that by some mischance the uterus becomes inverted, with a morbidly adherent placenta, should the placenta be separated before the uterus is returned, or should both be returned and the placenta thus be peeled off the womb? If only partially adherent remove the placenta at once, but if completely adherent return the whole. By this plan the placental site is less exposed to the air, and there is less hemorrhage. The attached placenta saves the uterus from bruising, and the sooner an inverted uterus is returned the better, as every moment lost renders its return more difficult. (Dr. A. L. Kelly, p. 264.)

Irritable Uterus.—*Bromide of Ammonium.*—Bromide of ammonium has an almost magical power of allaying uterine irritation and pain, in cases in which pain alone is the disease. A smaller dose than ten grains should never be given; it may be given in this dose every one, two, or three hours according to circumstances. If given in cases of painful menstruation with increased flow it will also frequently arrest the excessive hemorrhage. (Dr. G. de G. Griffith, p. 330.)

Lactation.—*To cause a Flow of Milk in the Female Breast.*—Make use of Faradisation; that is, *localise the galvanic current* on the breast, instead of passing the current through distant parts or with the poles far asunder. The magneto-electric, voltaic, and other machines will do, not excepting Pulvermacher's portable chain battery of 60 or 120 elements excited by vinegar, but Dr. Skinner uses one made by Horne and Thornthwaite. The positive pole is to be pressed deep into the axilla, whilst the negative is lightly applied to the nipple and the areola. The current should only be such as is agreeable to the part, The poles are to be kept steadily in this position for two or three minutes, the two poles are then to be pressed into or as it were imbedded in the mamma on each side of the nipple, and to be raised and reimbedded, observing that both poles are raised and reimbedded together. This is to be done all round the nipple, and all over the breast, particularly on the upper surface; this may occupy two or three minutes, not more. It may be repeated daily on both breasts. In many cases which seemed hopeless this was followed by the secretion of milk. (Dr. T. Skinner, p. 279.)

Ovariotomy.—The temperature of the room should be raised to 75°. It is better to operate without chloroform on account of the violent retching often following the use of it, but females often insist on the use of this drug, and we are obliged to consent. The large incision is to be preferred to the small one. It is better to have a bold and large incision, at once affording plenty of room for easy manipulation, aided by the eye, instead of dragging cysts or solid masses through small openings without a knowledge of what attachments may possibly exist behind unseen, and unfelt by the finger. (Dr. C. Clay, p. 280.)

In performing the operation of ovariotomy it is only necessary to make an incision two or three inches in length through the abdominal parietes, and it may afterwards be enlarged if necessary. The operator must not cut down without a director however carefully he does it, for it occasionally happens that a knuckle of intestine is held by adhesions in front of the tumour, and it would run great danger of being cut. The best position for the operation is between the legs of the patient, the thighs being held apart by assistants.' After removing the tumour never fail to examine the other ovary, for it is frequently diseased, if so, it is better to tie the pedicle and remove the ovary. Every clot of *blood* must be removed from the cavity of the abdomen, but simple fluid from the cyst need not be removed if small in quantity, because, sponging irritates the peritoneum. To avoid the escape of blood into the peritoneum during the operation, an assistant is required on each side to grasp the edges of the wound with warm flannels, and to hold them everted as the incision is made. If the wound bleed, the blood comes into the flannel; and if the intestines rise up the flannels keep them back. The bandage used after the operation must be one which can be slackened or tightened at pleasure, otherwise the patient will suffer great discomfort. It is as well to have it in one piece fastening in front by pieces of tape, which can be tied or untied at pleasure without disturbing the patient. (Mr. I. Baker Brown, p. 298.)

PESSARIES.—Dr. Clay has contrived to support the womb by simple wire loops bent into various shapes, so as to be adapted to most of the malpositions of the womb. We cannot describe the shape of these loops but refer the reader to the woodcuts at page 254. (Dr. C. Clay, p. 253.)

PROLAPSE OF THE FUNIS.—Pass the cord up to the fundus of the uterus enclosed in the hand, and hook it over the highest portion of the fœtus. This may easily be done during the absence of a pain. (Sir R. Crofts, Dr. J. L. Earle, p. 277.)

RECTIFICATION OF OCCIPITO-POSTERIOR POSITION OF THE FŒTAL HEAD.—There is no doubt but that rectification of an occipito-posterior position of the fœtal head may be accomplished by the accoucheur in many cases. When the head is free above the brim it can be done by grasping the head with long straight forceps, which must be gently turned until the head is brought into the desired position, the handles being at the same time pressed back against the perineum. Such revolution sometimes occurs spontaneously. It is rare, however, that circumstances call for interference at this stage of labour. When the head has descended to the floor of the pelvis, should rotation not spontaneously occur, (which happens under ordinary circumstances) we must interfere by a combination of manual and instrumental forces, if manual force alone does not succeed. By using the vectis and the fingers we can often succeed in causing the forehead to move upwards and backwards, and rotation to occur. The ear must be brought quite to the side, and the anterior fontanelle raised during the absence of a pain, traction being of course made with the vectis on the occiput. (Dr. W. Leishman, p. 269.)

RUPTURED PERINEUM.—In performing a plastic operation for ruptured perineum the following points are of importance. Before commencing the incisions, place the left forefinger on the mucous membrane to mark exactly the extent of incision intended. When the sphincter has been lacerated carefully bare the mucous membrane of the bowel, but do not cut it through. Always remove any cicatrised bands, Nature's attempts at union, which may be formed in the injured parts. Never stop in the operation on account of hemorrhage. Use a needle much longer than the one ordinarily employed, so that so great a sweep may be made that what usually requires two stitches may be done in one. When the cut surfaces are brought in apposition all bleeding will cease. When the sphincter is torn through, the superficial fibres on each side must always be divided on *both* sides (for the sphincter is a double muscle) otherwise there is an injurious strain upon the apposed parts. (Mr. I. B. Brown, p. 322.)

SHORT *versus* LONG FORCEPS.—In every case in which the long forceps are employed the short ones may quite as readily and effectually be employed. The long forceps may without injury to practical obstetricy be obliterated from the list of midwifery instruments. (Dr. J. Thompson, p. 255.)

UTERINE PORTE-CAUSTIQUE.—A very simple and efficient little instrument is made and used thus : A long silver probe with an olive-shaped enlargement an inch and a-half from its extremity must be attached to a handle of convenient length. Having bent it to the curve required by the particular case, arm it with the nitrate of silver by melting the latter in any convenient vessel. The end must be cleaned thoroughly before it is introduced, that none of the caustic may enter the uterus. (Dr. F. D. Lente, p. 346.)

Vesico-Vaginal Fistula.—In paring the edges of the fistula, it is most essential that the mucous membrane of the vagina should alone be removed to the extreme margin of the opening, and that the walls of the bladder be left untouched by the knife; so that, although the opening in the vaginal wall is enlarged when the edges are pared, the opening in the vesical wall is no larger than previous to the operation. (Mr. G. Pollock, p. 315.)

MISCELLANEA.

Bromide of Potassium.—Bromide of potassium has an action distinct from the iodide. It has what may be called an alterative action, relieving certain forms of chronic disease, as syphilitic skin affections. It exerts a most powerful influence on the generative organs, lowering their functions in a remarkable degree. It hence possesses valuable powers in diseases dependent on, and accompanied by excitement or over-action of the generative organs. Sir Charles Locock uses it in hysterical epilepsy and other nervous affections connected with uterine disturbance. It may be given with advantage in nymphomania, priapism, and in certain forms of menorrhagia. Lastly, it appears to produce an anæsthetic condition of the larynx and pharynx; for this purpose the salt of potassium acts equally well with the salt of ammonium. *Bromides* act chiefly on the nervous system, *iodides* on the mucous membranes and secreting organs. (Dr. A. B. Garrod, p. 378.)

Death from Chloroform.—*Cause of.*—It is of very little use watching the pulse in cases in which chloroform is administered, for the first step towards death from chloroform is not from paralysis of the heart, but from paralysis of the diaphragm. It is the respiration, not the circulation which must be watched. The pulse is merely a reflex indication that respiration is right. Fatty heart has nothing to do with death from chloroform; nor does even valvular disease contraindicate its use, as has been shown by hundreds of instances. (Dr. C. Kidd, p. 379.)

Digitalis.—This drug is a tonic, not to the heart alone, but to the organic muscular system generally. It is a most valuable remedy when given in the treatment of dilatation, and is dangerous only in hypertrophy. It hence requires to be given with great caution in aortic regurgitant disease. (Dr. Fuller, Dr. Sutton, Dr. Wilks, p. 70.)

Exploratory Punctures.—Dr. Alexander Wood's syringes for subcutaneous injections may be used for the purpose of exploratory punctures, as thereby a considerable quantity of fluid can thus be obtained for examination. Small cysts, such as ganglia, may also be emptied in this way; and afterwards, if necessary, injected with iodine. (Dr. J. Brisbane, p. 304.)

LACTUCARIUM, OR EXTRACT OF LETTUCE.—Lettuce has not been included in the materia medica list of the British Pharmacopœia. It is almost, or quite inert as an anodyne. As much as sixty grains of lactucarium, or extract of lettuce, fails to produce any relief to pain. (Dr. A. B. Garrod, p. 367.)

READY-MADE PLASTERS.—Boil 100 or 150 grains of common starch in one ounce of glycerine. You have now a stiff glutinous compound which sticks close to the skin, and can be easily removed and re-applied. Instead of ordering belladonna or opium plasters, mix three grains of sulphate of atropia with a few drops of glycerine, and then incorporate it with one ounce of this stiff glycerine ointment, and spread it thickly on gutta percha cloth or oil-silk. Morphia and other alkaloids can be mixed in the same way. (Dr. Tilt, p. 353.)

SALTS OF MORPHIA.—The hydrochlorate is the only salt of morphia introduced in the British Pharmacopœia, the acetate being omitted. The reasons are, that the hydrochlorate is readily manufactured, forms very beautiful crystals, of a definite composition, and is not liable to change on exposure to the air; whereas the acetate is very difficult to crystallise, and is a salt liable to decompose, to lose some of its acetic acid, and to become much less soluble. (Dr. A. B. Garrod, p. 363.)

VERATRUM VIRIDE.—This drug certainly can depress most thoroughly, but it cannot be used as an arterial sedative without depressing the vital powers. (Dr. J. Watson, Dublin Medical Press, March 2, 1864, p. 223.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON THE PREVALENCE OF FEVERS IN LARGE TOWNS.

By Dr. ROBERT CHRISTISON, F.R.S.E., Professor of Materia Medica in the University of Edinburgh.

[Dr. Christison, in his address on Public Health delivered before the National Association for the promotion of Social Science, at Edinburgh, agrees with the leading London authorities on the subject of fever, in the four forms of fever peculiar to this country. Speaking of Relapsing (inflammatory) fever, Dr. Christison observes that the infection is not a virulent one, and that the progress of this fever by infection may be entirely prevented.]

Healthy persons in communication with fevers of this kind accumulated in an hospital ward, or lying in less numbers in their own small unventilated chambers, are seized almost certainly if they remain long enough, and are not shielded by a previous attack. But from a single case of this fever, in a middle-sized, well-aired room, it is never communicated to the healthy. With these facts before us, of which I could furnish pointed proof, were there time, it is evident theoretically how such a fever is to be extinguished. The favourite panacea of the present day for the prevention of all fevers—thorough drainage—is not the remedy. The best drainage leaves untouched the real foundation of the disease—viz., penury pent up in airless dwellings. But provide work for the unemployed, obtain from them in return due ventilation and cleanliness, and the epidemic will soon vanish. First, the new condition of things will make its infectious power harmless, and erelong it will cease to arise by spontaneous generation.

The carrying out of this theory into practice is, however, a formidable difficulty. How is work to be obtained in hard times for the unemployed? And, still more, how are the labouring classes to be taught the habit of ventilating their apartments? Success must depend on the resources, faith, and energy of a sympathising community, and upon the convictions of its suffering portion. But, at all events, we have a fundamental principle of social economy firmly established—that no epidemic of inflammatory fever can long withstand employment of the workman and fresh air in his house.

Typhus, simple typhus, nervous fever, low fever, putrid fever—a disease so familiar as to need no description here, though very different in form from the last—presents many agreements in those characters which give them both interest in the eyes of the cultivator of social science. It has its epidemic visitations; and fearful ones we have seen all over Britain, especially in this city, where there have been no fewer than five during the last forty-five years—the last and worst having occurred between 1847 and 1849. It puts on the epidemic shape only at periods of want among the labouring classes. Howsoever it may arise in the first instance, it spreads by infection; but its infection is not intense, hence adequate space and ventilation make it innocuous in that way, in so great a degree as to have misled good observers, and made them doubt its infectious property. So far typhus agrees with inflammatory fever. But, *firstly*, it is far more deadly, one in ten being the probable average of deaths from it. *Secondly*, it occurs at all times, and not merely at epidemic seasons. Seldom does a week pass in a large town like Edinburgh, without one or more deaths from typhus appearing on the register. *Thirdly*, in these non-epidemic periods it is met with among the rich as well as the poor, and perhaps in as great a proportion to their relative numbers.

Nothing is known of the origin of typhus in non-epidemic periods. Infection will not explain the occurrence of such fevers—not those, at least, which show themselves in the easy ranks of life. With the most ordinary care, cases of it in that circle do not reproduce it in the exposed; whence, then, could it reach themselves by communication, who had not had any exposure to it within their knowledge? Neither does it originate, at least generally, in faulty drainage, or other sources of foul air. In the metropolis, indeed, it is at present a prevailing opinion—much in favour also, as I understand, in this Association—that the sources of foul air are likewise the sources of typhus; but such a rule will not apply in Edinburgh. Foul air undoubtedly favours the spread of both typhus and inflammatory fevers in their epidemic visitations; for, setting aside its possible operation in other less ascertained ways, it implies confined air, want of ventilation, and therefore concentrated infection. I do not mean to deny that foul air of some kinds may sometimes simply cause typhus; but there must be better proofs than now exist, before this can be admitted as the constant or even general fact. Foul air will not account for the origin of the scattered (sporadic) cases of typhus in non-epidemic times. As little will air, merely foul, account for either the rise or fall of epidemics. Every physician of experience in this city has repeatedly seen in a family a solitary case of well-marked typhus, which no skill could trace to foul air in any shape, or from any source. No one ever heard here of the spread of typhus in the epidemic form being referable to an increase of foul air, apart from the resulting concentration of infectious effluvia from those ill with the disease. We have had within the last few years an instance of the fall of a great epidemic, the worst on record, without any commensurate amelioration of air, drainage, or other branch of cleanliness. This incident is so remarkable in its circumstances as to deserve careful consideration.

The annexed table shows the variations of fever in Edinburgh year by year since the century began, as derived from the records of the Royal Infirmary:—

12 mon. to Dec. 31,	1800,	. 329	12 mon. to Dec. 31,	1832,	. 1394
... ...	1801,	. 161	... ...	1833,	. 878
... ...	1802,	. 156	... ...	1834,	. 690
... ...	1803,	. 232	... ...	1835,	. 826
... ...	1804,	. 323	... ...	1836,	. 652
... ...	1805,	. 175	... ...	1837,	. 1224
... ...	1806,	. 95	... ...	1838,	. 2244
... ...	1807,	. 110	... ...	1839,	. 1235
... ...	1808,	. 111	... ...	1840,	. 782
... ...	1809,	. 186	... ...	1841,	. 1372
... ...	1810,	. 143	... ...	1842,	. 842
... ...	1811,	. 96	... ...	1843,	. 2080
... ...	1812,	. 103	9 mon. to Oct. 1,	1844,	. 3339
... ...	1813,	. 75	12 mon. to Oct. 1,	1845,	. 683
... ...	1814,	. 87	... ...	1846,	. 693
... ...	1815,	. 96	... ...	1847,	. 3688
... ...	1816,	. 105	... ...	1848,	. 4693
... ...	1817,	. 485	... ...	1849,	. 726
... ...	1818,	. 1546	... ...	1850,	. 520
... ...	1819,	. 1088	... ...	1851,	. 959
... ...	1820,	. 638	... ...	1852,	. 691
... ...	1821,	. 327	... ...	1853,	. 574
... ...	1822,	. 355	... ...	1854,	. 168
... ...	1823,	. 102	... ...	1855,	. 201
... ...	1824,	. 177	... ...	1856,	. 180
... ...	1825,	. 341	... ...	1857,	. 132
9 mon. to Oct. 1,	1826,	. 450	... ...	1858,	. 111
12 mon. to Dec. 31,	1827,	. 1875	... ...	1859,	. 183
... ...	1828,	. 2013	... ...	1860,	. 152
... ...	1829,	. 771	... ...	1861,	. 122
... ...	1830,	. 346	... ...	1862,	. 136
... ...	1831,	. 758	... ...	1863,	. 196

From this table it appears that after 1816, for a period of thirty-six years, the continued fevers of Edinburgh, of which typhus formed the largest proportion, seldom fell short in the Edinburgh Infirmary alone of 500 in any one year. In 1818 and 1819, the annual average reached 1300; in 1827 and 1828, nearly 2000; in 1837, 1838, and 1839, nearly 1600; in 1843 and 1844, above 2700; and in 1847 and 1848, 4200. It then suddenly fell next year to about 700, and kept that average for five years. But in 1854 it sank again abruptly to 170; since then the annual fevers have never exceeded 200. The average for the last ten years has been 158. Last year there were only 136; and this year, which ends for the hospital statistics on 1st October, there were 196. Until 1860, the statistics of the Infirmary did not distinguish the several forms of fever from one another. In 1847 and 1848, however, the annual average of typhus could not have fallen short of 2500; and after that it must have been between 400 and 500 until the year 1854. But in 1860 the number was accurately ascertained to be 67; in 1861, it was 50; in 1862, 14; and in 1863, 74. I am further indebted to the medical officer of the city, Dr. Littlejohn, for the fact that the deaths from typhus for the whole town registered in nine months since 1st January, have been 18. According to the average mortality of typhus, this number indicates 240 cases of typhus for the present year.

Now, about the period of this decrease, the drainage of that part of the city where the chief nests of fever always lay was improved. But the decrease began decidedly before the commencement of that reform. I am informed by our superintendent of drainage, that the works for improved drainage of the worst part of the city—the Grassmarket, Cowgate, Canongate, High Street, and the closes communicating with these—were only begun in 1854, were far advanced only in 1858, and are now all but finished. Neither can the rapid decrease of fever be ascribed to any satisfactory improvement in the cleaning of the lanes and houses of the working classes. I believe that long prior to the decrease, our police had done as much for the cleansing of the fever districts as the impracticable structure of the streets or lanes there, and the incorrigible habits of their occupants, would allow. And as for the home habits of these people, no such improvement of them has taken place in my time as will explain any other change of circumstances in their social ecomony.

Here, then, is a discovery which remains to be made in social science. Why is it that typhus, which had been almost a perpetual pestilence in Edinburgh for a third of a century, has been of late wearing itself out, and last year almost flattering us with its extinction? The cause has certainly not yet been discovered. My own strong impression is, that the secret will be found to be connected with the theory which has been much canvassed in the present day, the successive changes of type or constitution of epidemic diseases. But as this is a favourite theory of my own, I shall not here insist on it further than by warning all inquirers into the origin of zymotic diseases in foul miasms, that they run great risk of ascribing to these, and the removal of these, fluctuations in the prevalence of such diseases which are often far more probably owing to a more recondite cause—a change in epidemic constitution. In the meantime, the experience of the physicians of Edinburgh presents us with the precept, also derived from the experience of other great towns, though, perhaps, nowhere else so categorically, that typhus never can prevail in the epidemic form in face of employment for the working-classes and ventilation of their dwellings.

In the present line of inquiry, there is no occasion for noticing any other fever than enteric fever. Gastric fever, the only other sort arranged under the head of continued fevers, is not generally acknowledged at present by systematic authors. If it be a separate fever, it is one which, according to my own observation, occurs chiefly in the easy and wealthy ranks of life, and owes its origin mainly to high living and over-indulgence in the excitements of society, whether of the nature of business, study, or amusement. We have little to do with it therefore. Enteric fever is very differently circumstanced.

This disease, variously called dothinenteritis, entero-mesenteric fever, enteric typhus, typhoid fever, but most conveniently Enteric (bowel) fever, is the most deadly of all forms of continued fever. It was first distinguished from others in Germany in 1763, and was first accurately described in France by Bretonneau in 1812. It began to be distinguished from others in London about the close of the first quarter of the present century, and first of all by Dr. Bright in 1827. A little later, a few cases occurring in hospital here attracted great attention. It has since increased in both cities, as well as in Britain at large. In London it has been common for many years, and is sometimes the commonest of all forms of fever. In Edinburgh its course has been very singular, and deserves careful attention with reference to English opinion as to its cause, and the sanitary conclusions to which that opinion leads.

For many years after Bright wrote about it as a frequent fever in London, we saw it seldom here—never in the proper inhabitants of the city, but only in persons brought ill with it from Linlithgowshire or Fife, or who had quite recently left these counties. It began to be studied here with great care on its being recognised in the Infirmary in 1847, in a German lad only six months from his "Vaterland," on which occasion it was believed that no case had occurred in the hospital for a period of five years. By-and-by it became not uncommon. For some years past every practitioner has met with it. It occurs among old residenters and natives of the city. Within a few years it is encountered even among people in easy circumstances, and in the best houses of the town. In our Infirmary statistics, it was not taken account of separately from other fevers till 1860. In that year, according to a table supplied to me by Mr. Macdougall, superintendent of the hospital, there were 41 cases of enteric fever; in 1861, 35; in 1862, 79; and in 1863, 67; during which period the fevers of all kinds did not exceed 150 in any single year till the present, when they reached 196. These are no great numbers, and yet sufficient to show a decided tendency to increase during the last fifteen years. Further, Dr. Littlejohn, medical health officer for the city, informs me there have been seventeen deaths from enteric fever reported to the registrar since the 1st of January last, which will correspond with about 120 cases in twelve months, if the average mortality be taken at one in five.

Of all forms of fever none has been more confidently ascribed than this, by London writers, medical and non-medical, to faulty drainage and faulty provision of water-closets. If we are to believe what some have advanced on the subject, there is no case which may not be traced to foul air, derived mainly from one of these sources. Were this a well established principle in social science, the extinction of so deadly a fever should be no very difficult matter. Through the publicity given to the discovery by this Association, and the influence of its members, we might hope to see protection established far and wide against the pestilence.

But I am sorry I cannot call on you to assent to this theory, and carry out its consequences; for there are insurmountable facts in its way. During the period that this scourge has been alternately growing and diminishing in London, has London become alternately worse and better drained, or have the habits of its working-classes been alternately less and more cleanly? Does the disease generally appear where drainage is bad, or water-closets wanting or faultily constructed? Does it attack workmen who live in the London drains, as well as those over them, near them, or far from them? I believe all these queries must be answered in the negative. And what is the case here? Our street drains in the Old Town have been much improved during the very period that enteric fevers have been increasing. The habits of the working-classes in regard to cleanliness admit of reform undoubtedly, but certainly they have not been growing worse. Besides, the individual cases which have been occurring here have been made the subject of careful inquiry, and in many of them it has been impossible to discover any peculiar source of foul air—anything different from what may be met with in thousands of dwellings where this fever has never shown itself. Of the seventeen deaths hitherto this year, nine occurred in localities to which no objection could be found. And what are we to say of its appearance among people of easy circumstances? In this sphere I have myself known several deaths from it during the last few years, and no fewer than three during the last twelve months. In the first place, why has it occurred in that class of society only of late? Surely not from any general increase in defective drainage, defective water-closets, or other sources of uncleanliness. As to the history of individual cases, I have been content to ascribe the disease, in obedience to the theory of London writers, in one instance to gross disregard of ventilation, and in another to a faulty water-closet. But in the last three I have seen, no fault could be found anywhere. Further, this fever does not by any means generally break out where the streets are ill-drained, water-closets wanting, and habits filthy. In countless places of that sort in Edinburgh it is unknown. It may be worth while adding, in reference to an independent question likely to be discussed in this section, that enteric fever is not known in or near the "foul meadows" of Craigentinny.

I suspect, then, it must be allowed of this disease, as in respect to most other epidemic diseases, that we do not yet know its cause—that foul air merely favours its invasion; but that its true cause is something much more specific—some ζυμη, or ferment, which has hitherto eluded our search. In that case, while we shall do well to encourage better drains, more and better water-closets, and better ventilation of dwellings, still we must not count upon thus extirpating enteric fever.—*Edinburgh Medical Journal, Nov.* 1863, *p.* 429.

2.—ON THE CHLORINE TREATMENT OF SCARLET FEVER.

By E. J. Blyth, Esq., Whitchurch, Reading.

[Mr. Blyth about twelve months ago published an article on the employment of Chlorine in the treatment of Scarlet Fever. Since then he has had many opportunities of trying it, and only with the result of increasing his confidence in its utility in this disease. He says:]

If we consider the essence, so to say, of the disease, it must be due to a poisoned state of the blood, which poison takes some time to be diffused or multiplied in that fluid, and then the early symptoms of general fever—shivering, headache, sickness, &c.—occur, the pulse early being very frequent. Then nature endeavours to relieve herself by the characteristic eruption. In the onset it appears in the throat (internal skin) and is apparent there first owing to this very delicate internal skin being kept so warm and moist; then the eruption comes out on the skin generally. The mischief in the throat may be so great as to lead to ulceration and destruction of its tissue, sometimes the whole soft palate being lost; and this is from the poison destroying the nutrition of the part. In favourable cases generally the fever abates on the appearance of the rash. In unfavourable ones the system becomes poisoned more and more, and the patient succumbs. And this result is not confined to the severe cases, but the mild are fraught with danger; for when the skin does not come out in a good rash, so as to eliminate the poison, the kidneys make an effort to rid the blood of the offending material: their structure suffers, and dropsy and its sequences result. Bearing in mind that even in the mildest form of cases suddenly adverse symptoms occur,—as headache from cerebral congestion, rapidly proving fatal, or dropsy, which is frequently most obstinate and difficult to be managed,—I never depart from my rule of giving medicine to every patient where there is the most remote suspicion that the case may prove one of scarlet fever. There is but small doubt that the infectious state is when the patient himself is getting all right; the disease has expended its force on him, and he is throwing it off, and is casting his skin. This attempt at excretion occurs early in the throat, and the excreted material from that surface should be rendered inert. I believe a great point in treatment is to give some remedy frequently, which in passing over the throat shall neutralize the poison which nature is casting off by the eruptions and secretions of the throat, and so prevent the poisoned matter being swallowed over and over again, and the system more and more loaded; and at the same time the frequent administration of the remedy may influence the whole mass of the blood beneficially, and that in the shortest time possible. My plan is to give the solution of chlorine every hour night and day. Where the case is anything like an anxious one,—and how anxious may even the most simple case become!—I rely solely on the chlorine. Very rarely I may perchance add a little antimony to lower the pulse; but, as a rule, I only give the chlorine, sometimes with quina, &c. &c.

Let me urge my medical brethren to give a full and fair trial to my remedy, relying implicitly on it; and this I ask of every one who does so, that he will send me every six months an account of his experience. I see no reason why every one using this remedy fully should not be blessed with the same success that I have been, during many years that I have used this specific in an active and extensive practice.

Put ten grains of chlorate of potash, finely levigated, in a pint bottle; cork it; add one drachm of hydrochloric acid; in a minute or so add an ounce of water, and shake; to dissolve the vapour, let it stand a short time; then add more and more water at intervals until the bottle is filled. A child of twelve years may take one ounce every hour.—*Lancet, Jan.* 16, 1864, *p.* 87.

3.—ON THE USE OF SULPHITES AND HYPOSULPHITES IN THE TREATMENT OF ZYMOTIC DISEASES.

By Dr. De Ricci.

Professor Polli, of Milan, has, for several years past, devoted much of his time to the investigation of zymotic diseases in general, but more especially of those which depend upon purulent absorption, with a view, if possible, to discovering an agent which would be capable of destroying those fermenting principles which are supposed to be the cause of these diseases, while it would be, at the same time, innocuous to the constitution. In May, 1862, the Dublin Quarterly Journal of Medical Science published a short review on Dr. Polli's work *On the Use of Alkaline Sulphites in the Treatment of Diseases depending on Morbific Ferments*, and that valuable periodical fully recognised the importance of his researches. Since that time I have been in direct correspondence with the Italian professor, and, having received from himself much additional information, together with the request that I would prominently bring the subject of his investigations before the medical profession in this country, I have complied with his request, and do so at present by laying a brief sketch of his labours and results before the Obstetrical Society at Dublin.

I should have been happy if I could have brought additional personal evidence of my own to corroborate Dr. Polli's accounts, but, unfortunately, my opportunities have been very limited. I have, however, proved to my complete satisfaction that the sulphites of soda, potassa, and magnesia, which are the remedies he relies on in cases of purulent infection, can be freely administered to human beings, even in large doses and for a considerable time, without producing any disagreeable consequences; and this fact will, I trust, induce many to give a trial to these substances in every case they may appear to be indicated, in order to test their real value.

I do not now stop to argue whether these so-called zymotic diseases really depend upon the presence in the circulation of a special ferment, as their name would imply; it is generally thought to be so, and for the present we must take it for granted, as Professor Polli bases all his theory and practice upon that assumption. The great physiologist Bernard held this view, and believed "that fermentation may arise in the blood, and give origin to poisonous principles, which may, in their turn, produce certain grave accidents in the living frame;" but adds:—"We cannot neutralize these ferments in the living organism; *it is impossible;* because, to effect such a purpose, it would be necessary to interfere with the characters of the blood to such a degree that it would no longer be capable of maintaining life." The Italian professor, however, thought differently. He had long been studying the antiseptic effects of sulphurous acid upon animal substances; he had established, by repeated experiments, that animals recently killed, if suspended in a well-closed vessel, containing but a small quantity of a solution of sulphurous acid in water, just sufficient to supply, by evaporation, a slight atmosphere of sulphurous acid gas, would keep perfectly fresh for months. He also established, by direct experiment, that sulphurous acid, in very small quantities, had the power of preventing not only the ordinary vinous fermentations—a fact well known in all wine countries—but also those other fermentations, such as the diastatic, by which starch is converted into glucose, that of the pancreatic juice upon fatty substances, and that of emulsine upon amygdaline; and he came to the conclusion that in sulphurous acid we possess a substance capable of arresting every form of catalytic action. But sulphurous acid could not, with impunity, be introduced into the animal economy; so the professor turned his attention to the compounds of sulphurous and hyposulphurous acid, which, by analogy, he hoped to find equally capable, though probably in a minor degree, of arresting the action of ferments. He was not disappointed. He found that the action of the sulphites of soda, potassa, and magnesia was quite as decided as that of the acid itself in preventing fermentation; while, at the same time, he fully established the fact, that an animal, such as a dog, can take as much as fifteen grammes (225 grains) of an alkaline sulphite per diem, during a whole fortnight, without any inconvenience. I can fully corroborate the assertion of Dr. Polli so far; and am happy to be able to add my testimony to the fact of the perfect tolerance, by even the human stomach, of the sulphite of soda in full doses. I have often prescribed it, during the last two years, in doses of as much as one drachm three times a day, without producing the slightest gastric derangement; and, although I have not, as yet, had an opportunity of testing its value in a case of either pyemia, puerperal peritonitis, or glanders, still the results I obtained in the cases I employed it have led me to hope much, from its extended use in all diseases arising from purulent infection or septicemia.

Before relating, however, the results of my small experience in this matter, I wish to lay before the society some further account of Professor Polli's investigations. Having satisfactorily proved to himself that animals can bear with impunity large doses of sulphites, he took three dogs of about the same size; two of these he fed upon food containing sulphite of soda, the third he fed exactly alike, only minus the sulphite. After twenty-four hours the three animals were killed, and, by analysis, he discovered the presence of the sulphite in the blood, the liver, and the urine of the first two dogs, while, as might have been anticipated, he found none in the third. This was a very valuable experiment. It proved that the sulphites, *as such*, were carried into the circulation; and, if they were able to prevent and even arrest the action of an animal ferment outside the body, Professor Polli argued that they might equally prevent or arrest the action of a ferment within the body; and, as the dogs which had been largely dosed with sulphites had shown no bad effects from it during life, nor any organic lesion from the use of them when examined after death, they having been purposely killed while still under the full influence of the sulphites, he concluded that in all probability we possessed in the compounds of sulphurous acid, with earths and alkalies, a means of arresting catalytic action in the animal economy without in any way interfering with the vitality of the blood—contrary to Claude Bernard's assertion, that any substance capable of destroying the action of a ferment in the living body would exert so destructive an action on the blood itself, as to imperil its vitality. Professor Polli now varied his experiments. He administered two grammes of sulphite of soda daily to a dog, for the space of five days; at the end of that time he drew off two ounces of blood, and exposed it to the air in an open vessel, beside another vessel containing blood drawn from a dog to whom no sulphites had been given. In a very few days the blood of the latter dog was quite putrid, while that of the dog which had been dosed with the sulphites was perfectly fresh even three weeks later. Two great facts had now been arrived at:—1st. That sulphites, when administered to a living animal, are carried, *as such*, into the circulation, and diffused all over the organism without the slightest inconvenience to the animal; and, 2nd. That the presence of these salts in the liquids and solids of the body retards the putrefactive fermentation for a very considerable period.

Dr. Polli having obtained such encouraging results, proceeded to more crucial experiments. He took two dogs of about the same size, and equally in good health; he fed them exactly alike for five days, with the exception of administering to one of them two grammes of sulphite of soda daily—the other dog getting exactly the same food, minus the sulphite. At the end of five days he injected into the femoral veins of both animals one gramme of pus, taken from a fetid abscess occurring in a broken-down constitution. The operation, in both cases, was carefully performed, and the animals suffered but little. Immediately after the injection both dogs appeared stupefied; they lay down and refused all food, remaining quite prostrate for twenty-four hours. On the following day, however, they both seemed a little better, and took some food. A second injection of pus was now practised on both animals to the same amount, but the first dog had, the meanwhile, been getting two grammes of sulphite daily, while the other was only getting plain food. The effect of the second injection was most interesting; both dogs were affected instantly alike; both were seized with stupor; in both the pulse was rapid, but feeble, while the respiration was greatly accelerated. Both dogs refused to eat; both lay down in a state of stupor; and, when made to rise and walk, they tottered and reeled across the room. The first dog, however, continued to receive, daily, a dose of two grammes of sulphite of soda, and in four days was so far recovered as to be able to eat his food with relish, while the wound in the femoral vein was rapidly healing. The other dog fared differently; he got no sulphites, either before or after the operation, and the result was that he daily became worse; the wound in the thigh became gangrenous, the limb swelled up, and ten days after the second injection the dog died with all the symptoms of typhus, the first dog being already about and well. The result of this experiment was highly satisfactory; but the Professor did not rest satisfied with it, so he again proceeded as follows:—On the 9th of March, 1861, he injected into the femoral veins of three dogs three grammes of putrid blood to each. (This blood was defibrinated bullock's blood which had been kept for four months; it was quite putrid, and exhaled a highly fetid ammoniacal odour.) The three dogs were about the same size, and were fed exactly alike, with the exception of dog No. 1, who, for two days previous to the operation, had taken sixteen grammes of sulphite of soda in one gramme doses. A few minutes after the injection the three dogs were affected very much alike. No. 1 vomited almost immediately, looked ill, lay down at once, and for several hours refused all food; the following day it was still heavy and stupid. Two grammes of sulphite of soda were administered to it, and repeated the following days; on the third after the operation the dog was already much improved, and by the fifth it was perfectly well. Of the other two dogs, one, after having vomited, remained standing, with its legs wide apart and its head hanging down; it shook and shivered all over, then fell on its side, panting violently; gradually it became worse and worse, and finally died in five hours. The other suffered much in the same way; it lived five days, during which it was too weak to stand; it did not eat; the wound in the thigh became gangrenous, and it expired comatose, with all the symptoms of typhus fever. It was examined after death, when the lungs were found of a deep red colour, dotted all over with ecchymotic patches, some of which had suppurated in the centre, the right cavity of the heart was filled with black grumous blood, while a pale yellow fibrinous clot filled not only the left ventricle, but extended also into the aorta. The whole gastro-intestinal tract was injected, and smeared in places by purulent matter.

This same experiment was now repeated in a modified form. Two dogs of nearly same weight, in good health, were submitted to an injection of one gramme of putrid blood each; but in one case the blood was diluted first with three grammes of a saturated solution of sulphite of soda. Both dogs were very ill at first, and both recovered eventually; but while the dog which had received the diluted injection was only sick for two days, the other was more than a fortnight before it struggled into convalescence.

This was a very valuable experiment. It showed that these sulphites can with safety be introduced directly into the circulation, without in any way endangering the vitality of the blood. I should have premised that Dr. Polli had previously tested the safety of injecting a solution of sulphite of soda into the circulation by trying it in healthy dogs, and had done so repeatedly without any bad results.

Encouraged by the evident success of his experiments, Dr. Polli determined to test the efficacy of the sulphites by bringing them into direct antagonism with one of the most virulent of animal poisons, that of glanders. He took a strong, healthy dog, and, having made a cutaneous incision between its shoulders, where the animal could not reach with its mouth, he introduced through it into the subcutaneous cellular tissue, some discharge obtained from the nares of a glandered horse. In a few days the wound became gaping and ill-conditioned; the dog looked ill, heavy, and stupid; it refused its food; and, by the fourteenth day, the animal had a number of unhealthy pustules over his body, which spread out into ill-looking, sanious ulcers. The dog became daily worse; and finally died twenty-six days after the inoculation. An examination after death revealed a highly injected condition of the mucous membrane of the stomach, with many ecchymotic patches. The intestines were highly vascular, and of a dark livid colour; while the lungs were thickly studded with apoplectic clots. Professor Polli now took two large dogs, as similar as possible both in size and health, and having administered to one of them eight grammes daily of sulphite of soda, he injected into the femoral veins of both dogs three grammes each of the muco-purulent discharge obtained from the nares of the same glandered horse which had served for the previous experiment. The first dog which had received the sulphites seemed at first to suffer the most from the injection. It at once fell to the ground as stunned, and its breathing was rapid and panting; but in a few hours it began to recover, and the following day it was able to eat. The second dog bore the operation better, and did not appear to sustain so severe a shock; but on the following day it began to mope; towards evening it was very drowsy, and with difficulty it could be got to stand; by the third day the animal's extremities had become œdematous and painful; by the fourth, a purulent discharge was running from its nose and eyes—the wound in the thigh was now almost gangrenous; on the sixth day the animal died, worn out by pain, fetid suppuration, and diarrhœa. The first dog was by this time completely recovered.

I might relate many other instances of equal interest, as Professor Polli has repeated these experiments nearly a hundred times, *almost always* with a *successful*, *always invariably* with a *satisfactory* result. He seems to have clearly established that, contrary to C. Bernard's assertion, those ferments which, in the animal frame, are capable of originating zymotic diseases, *can be neutralized* by substances which do not in any way prove injurious to the animal economy. I cannot bear full testimony to the correctness of Professor Polli's experiments, not having as yet had as ample opportunities of testing their accuracy as I could wish; but I can bear testimony to the harmlessness of the sulphites of soda, potassa, and magnesia, when administered internally, even in large doses; and I think that in three cases in which I employed these remedies I observed a decided improvement after their administration. One was a case of phthisis, with excessive purulent expectoration. The patient took for several months one scruple of sulphite of soda three times a-day, with very manifest advantage. In this case, although the cure was hopeless, I gave the sulphite with a view to diminishing the purulent secretion, and I was not disappointed. Another case was that of a gentleman who consulted me about an unpleasant eruption by which he was tormented, and which looked extremely like rupia. He assured me, however, that he had not had syphilis for eighteen years previous; and I have no reason to doubt his assertion. I gave him half drachm doses of sulphite of soda three times a-day, in a bitter infusion, and in a short time he became perfectly well. I gave him no other medicine. The third case was one of constantly recurring boils, some of which were sufficiently large to require incisions. The patient was a young gentleman in easy circumstances, well nourished; and although a student in an English university, not in any way broken in health by over study. He had been suffering from these troublesome boils for upwards of six months. After trying divers remedies, I placed him under a course of sulphite of soda, giving him one drachm of the salt three times a-day; and in less than a month not a boil could be seen on his body, though his face, neck, and shoulders still exhibited plentiful traces of his former tormentors.

It is not in private practice, however, that the real value of a remedy can be fairly tested. I have proved to my satisfaction that the sulphites are, at any rate, uninjurious to the animal economy. It now becomes the duty of those physicians and surgeons who enjoy the incomparable advantage of hospital practice to put these remedies to the test. It is to them that my friend, Dr. Polli, appeals. "To the test of clinical investigation and clinical result I leave the issue of my discovery," are his words to me.

If Professor Polli has not deceived himself—if these sulphites really have the power of neutralizing animal poisons, even after their absorption into the circulation—his discovery would be as great, if not greater, than that of Jenner. Nothing can be more candid than his conduct has been throughout. He has made no secret of his discovery; and nothing can be more modest than his constant expression:—"I wait for the verdict of the clinical students of Europe."

In conclusion, I earnestly beg to call the attention of all practitioners, and especially of those who enjoy the privilege of hospital practice, to the subject of this paper. Let these remedies be tried in every case in which they offer a chance of success, both in the treatment of disease and as prophylactics also. Whenever they meet a case of scarlatina, let them treat not only the patient, but let every individual in the family take a certain quantity daily of one of these sulphites; and let the same plan be adopted in every case apparently depending on some zymotic poison, whether fever, pyæmia, septicemia, or puerperal peritonitis.—*Dublin Quarterly Journal, Nov.* 1863, *p.* 470.

4.—A CASE OF GONORRHŒAL RHEUMATISM SUCCESSFULLY TREATED WITH THE TINCTURE OF LARCH.

By Dr. John Popham, A.M., Physician to the Cork North Infirmary.

[The patient was completely disabled from rheumatism affecting nearly every joint of his body. Six weeks before admission to the Cork Infirmary he had been wet through, and exposed to severe cold while suffering from gonorrhœa. Severe rigors and pains in the joints ensued.]

The chief joints engaged were the shoulders and elbows of the upper extremities, and the hips, knees and ankles of the lower. From the wasting of the muscular and adipose tissues, the joints seemed so much enlarged as to present an appearance of considerable deformity. The ankle joints were much swollen and painful, and the soles of the feet were covered with a thick scaly incrustation, extending round to the roots of the large toe nails. The calves of the legs were thin, flabby, and pendulous. He had a urethral discharge of a gleety character. From the severity of the nocturnal pains and the loss of sleep his face had a haggard and greasy or waxy look. The tongue and throat were covered with a curdy-looking coat, but without any ulceration. His pulse varied from seventy to eighty; he had no cough or pain of the chest, but his voice was weak, and at times scarcely audible.

In consequence of the nocturnal pains he was ordered the iodide of potassium in decoction of bark, and an opiate at bedtime, accompanied with the cautious use of vapour baths, both general and local, with a diet of meat and porter. The further progress of the case was as follows:—

Sept. 14. Symptoms as before. As he was greatly emaciated he was directed to try a dessert spoonful of cod-liver oil three times a-day.

Sept. 19. The bark was changed for sarsaparilla, as it caused nausea.

Sept. 27. A troublesome diarrhœa set in, which he attributed to the porter, which was indifferent. His medicines were stopped, and he was ordered chalk mixture with catechu and opium, and a full dose of Dover's powder at bed-time.

Oct. 8. The diarrhœa has ceased. Ordered a diet of milk and eggs, lime-water to be used with the milk; opiates at bed-time, and a grain of sulphate of quinine three times a-day.

Oct. 17. The night pains were so agonizing that the iodide of potassium was again tried; and a chloroform liniment gave some relief.

Oct. 24. The cod-liver oil was used for a few days, but was thrown off the stomach. On reviewing the case the progress seemed discouraging. The pains continued unabated; the scaly eruption on the feet was spreading; his general health was not improved; and the helpless and crippled state of the limbs persisted. It was plain that the course of treatment hitherto adopted was inefficacious: but what other could be substituted with advantage? On inquiry he admitted that, ten years before, he had a primary venereal affection, so that there appeared a possibility that gentle mercurialization might improve the scaly psoriasis. With this hope, half a drachm of mercurial ointment was rubbed in every second night, and washed off in the morning, so as to hold the remedy in check. Other medicines were discontinued, except a night dose of Dover's powder.

Oct. 30. He has had a return of diarrhœa during the last few days, for which he was ordered decoction of logwood, with tinctures of cinnamon and opium.

Nov. 10. The diarrhœa has ceased; the mouth has become slightly affected. Ordered to omit the inunction. As the Dover's powder produced perspirations which weakened him, he was ordered a night draught of tincture of hyoscyamus. The quinine was resumed, and he was directed to take a couple of glasses of sound wine daily, with strong beef tea *ad libitum*.

Nov. 21. Not much improvement as yet from the mercurial course; his state is very critical; his tongue is red, with a curdy layer of an aphthous character; the throat is of an erysipelatous hue; tenderness on pressing epigastrium; nausea, and occasional vomiting; aphonia almost complete; he speaks in so low a whisper that a person must stoop down to hear him: pulse not increased in frequency; stripping prevented by the use of several air pillows; he lies on the back altogether, and the bones are projecting. Upon a close examination of the chest no evidence of tubercular deposit could be detected. At this stage of the case he was ordered the larch tincture—half a drachm three times a day, and fifteen minims of Battley's sedative every night.

Dec. 12. He has taken the tincture of larch regularly since the preceding report, and has been getting gradually stronger. The larch does not seem to increase the quantity of urine, or to present any obvious effect. Besides the larch, he has continued the quinine as before, finding that it checked perspirations, and also the night draught of Battley. His diet is three pints of new milk per diem, eggs, and two or three glasses of port wine. As he still feels the pains of the joints, he was ordered a liniment of half an ounce of tincture of aconite in an ounce and a half of camphor liniment, which afforded him great relief.

1863, Jan. 17. Since the last report he has improved considerably; he is able to be up for a few hours daily; he is gaining flesh; the painful and desponding expression of his countenance has given place to hope; the squamous eruption upon the feet, which has been on the decline since the beginning of December, has now disappeared; he has recovered his voice; all the symptoms of gastro-enteritic disturbance have left him, so that he has been able to resume the use of meat. It was resolved to test the effect of the medicines by omitting the quinine.

Jan. 24. He is of opinion that, since the omission of the quinine, the perspirations are more copious at night; in every other way he continues to improve.

Feb. 2. Since the last report he has resumed the quinine, and with the effect of stopping the perspirations; the night pains have ceased; he gets about by the aid of two sticks, but when long up the ankles swell; he is gaining flesh and strength rapidly.

Feb. 26. He left the hospital this day, able to walk without support, and in every way recovered.

Remarks.—The preceding case afforded an instance of intractable arthritic affection, arising from protracted exposure to cold and wet, while the patient was suffering from chronic inflammation of the urethra. The arthritis is evidently not metastatic, as the urethral discharge did not abate; but it set in with much greater severity than occurs in ordinary arthritic attacks. For example:—The patient was much more worn down upon admission that he might reasonably be expected to have become in so short a time; his muscles had lost their tension and rapidity of contraction, and, in all probability, their vividness of colour; the joints, especially the ankles, had become enlarged by deposits consisting of effusion into the areolar tissue, and thickening of the periosteum and perichondrium. A very peculiar scaly eruption formed upon the soles of the feet, but rarely met with in such cases, and confined to those parts, so that it may be truly termed "plantar psoriasis." It is worthy of notice that the heart was not engaged, and seldom is, in this form. The usual nocturnal exacerbations were also unusually severe. Another peculiarity of this affection is a sallow, tallowy hue of the countenance, with a worn and despairing expression; and there is reason to suppose that the tendency to gastro-enteritic complication is not casual.

With respect to the treatment:—The iodide of potassium, usually so effective a remedy in syphilitic pains, failed to afford relief; the various preparations of bark were equally powerless, though quinine succeeded in checking the perspirations. All forms of baths increased the debility; sarsaparilla and guaiacum were inoperative, and cod-liver oil disagreed. The claims of the mercurial treatment cannot be disposed of so evidently; it indisputably caused much present disturbance; but whether it co-operated in overcoming slowly the deep resistance of the disease is open to question.

There is nothing more certain than that the amelioration in the case commenced with the administration of the larch. He was reduced to extreme emaciation, and only prevented from stripping by the ingenious application of air cushions disposed all round his person. All the gastric symptoms, one by one, retired; the curdy aphthous secretions, the raw redness of the pharynx, and the complete aphonia gradually yielded. The urethral discharge ceased, under injections, at an early period of the case, and did not seem to increase or lessen the arthritis while it lasted. As to the *modus agendi* of the larch—it did not act by diuresis, as some supposed; nor did it cause diarrhœa, as I have noticed in some cases where it has been given. It probably exerts some specific effect upon the urinary system, the urine becoming natural, apparently in its sensible qualities, under its use. Of the unmanageable nature of this ailment every practitioner of experience will recollect examples.—*Dublin Quarterly Journal, Nov.* 1863, *p.* 478.

5.—ON THE TREATMENT OF TENDINOUS RHEUMATISM BY THE EXTERNAL EMPLOYMENT OF SULPHUR.

By Dr. Renard.

Tendinous rheumatism, according to Dr. Renard, differs from acute rheumatism by the absence of the general symptoms, and from the chronic by the presence of local inflammatory symptoms. Dr. Renard suffered from this complaint himself after an attack of acute rheumatism, for which he was copiously bled. The parts affected were the tendons of the hamstring muscles, and no improvement resulted after a long course of diaphoretics, camphor, terebinthinate and other liniments, and the administration of the solanaceæ. At last Dr. Renard saw a passage in an English medical journal, stating that persons suffering from rheumatism in the legs had only to dust the inside of their stockings with sulphur. He immediately employed this simple remedy, the sulphur being the commercial flowers of brimstone, which contain some sulphurous acid. The curative effect was very well marked, for Dr. Renard walked in the evening, then renewed the sulphur in the stockings before sleeping in them, found himself very much relieved the next morning, and nearly quite cured on the morning after. A few days later, he left off the brimstone, and the pain reappeared in the soles of the feet, but yielded very soon to the reapplication of sulphur. Since the year 1857, when he was first attacked, the same experiment was repeated every winter when he was suffering from chronic tenodynia, either in the hams, the heels, or the elbows. He felt, under the influence of the contact of the flowers of brimstone, the skin becoming hotter, slightly excited, and more disposed to sweating; and as soon as this effect was produced, the relief of the pain seemed to be immediately marked. Whatever may be the explanation of the manner in which sulphur exerts its curative agency, Dr Renard affirms that it has a beneficial effect upon the rheumatic pains of the tendons, and that this action is the more rapid and certain in proportion as the tendons are more superficial and the sulphur is kept more closely over the painful parts.—*British and Foreign Medico-Chirurgical Review, Jan.* 1864, *p.* 241.

6.—ON DROPSY, ITS GENERAL PATHOLOGY, PROGNOSIS, AND PRINCIPLES OF TREATMENT.

By Dr. GEORGE JOHNSON, Physician to King's College Hospital, Professor of Medicine in King's College.

[The main object in this paper, which was delivered as a clinical lecture at King's College, is to give a clear and intelligible explanation of dropsical accumulations. It is evident that the excess of fluid in the serous cavities and in the meshes of areolar tissue, in which dropsy consists, must be owing either to excessive secretion or defective absorption, or to both combined. It is probable that while dropsical effusion is going on, the absorption of liquid from the dropsical parts is nearly if not quite suspended.]

So far as is known, the *bloodvessels* are alone concerned in the exhalation and absorption of this fluid. The lymphatic absorbents have no share in the phenomena of dropsy. The explanation of dropsy is to be found in the condition of the bloodvessels and their contents. In general terms, the proximate cause of dropsy may be, 1st, a mechanical impediment to the circulation of the blood, and a consequent over-fulness of some part of the vascular system; 2nd, an alteration of the physical condition of the blood—either (*a*) an excess of the watery part, with a relative deficiency of the solids, or (*b*) an accumulation of excrementitious materials, especially of urinary constituents.

Let us now take some simple examples illustrative of each of these conditions as a cause of dropsy.

An impeded return of the blood through the veins often causes dropsical swelling of the parts beyond the seat of obstruction. Thus the pressure of enlarged glands, or of an aneurism on the femoral or popliteal vein, or the pressure of the gravid uterus on the iliac veins, will give rise to oedema of the lower extremities. Structural disease of the liver, especially an advanced stage of cirrhosis, may greatly impede the return of blood through the portal system of veins, and the result is an effusion of serum into the cavity of the peritoneum, constituting ascites. Dr. Watson relates the case of a man who had dropsy of the head and neck and upper extremities, while the rest of the body was free from dropsical swelling, and disproportionately small. The cause of the dropsy was found to be obliteration of the superior cava vein by the pressure of an aneurism of the aorta. The larger the vein which is obstructed, the greater will be the extent of the dropsy; and when the impediment to the circulation exists at one of the orifices of the heart, the dropsy may be general over the whole body.

Now the immediate cause of the dropsical effusion in these cases is *over-distension of the veins and capillaries, and increased pressure on their walls.*

Experiment has shown that the transmission of fluids through membranes is in direct proportion to the *pressure* employed. Experiments by Magendie and others have also shown that while depletion of the bloodvessels by venesection quickens absorption, artificial distension of the vessels by the injection of water checks absorption and favours exhalation or the escape of liquid through the walls of the vessels.

Again, absorption is checked by a retarded movement of the blood through the vessels. "Fluid may be raised against gravity by directing a stream along a membranous canal which lies immersed in the stagnant fluid. The outer fluid enters the canal by endosmose, and is carried away with a speed proportioned to the velocity of the current." An obstacle to the return of blood through the veins therefore checks absorption and favours dropsical effusion, by causing a retarded circulation, increased fulness of the veins and capillaries, and increased pressure on their walls.

It is thus, chiefly, that valvular disease of the heart, and some forms of pulmonary disease—such as emphysema with bronchitis—cause dropsy. I shall presently show you that congestion of the kidneys and a consequent scanty secretion of urine are amongst the results of the cardiac and pulmonary disease which concur in causing dropsical effusion.

Watery blood.—We have next to speak of dropsy having its origin in an alteration of the physical condition of the blood. And, first, dropsy may result from an excess of water and a deficiency of the solid constituents of the blood. In cases of extreme anæmia, whether a result of hemorrhage or that condition of system to which the term chlorosis is applied, we frequently find more or less dropsical swelling, especially of the lower extremities. In these cases the escape of serum from the bloodvessels into the areolar tissue is accounted for partly by the thin and watery state of the blood, partly, perhaps, by an enfeebled condition of the heart. Palpitation and dyspnœa on exertion are common symptoms. It is probable, also, that this thin and watery blood is less readily transmitted through the capillaries than blood which has its normal consistence and composition. Anæmia, therefore, may tend to cause dropsical effusion through the influence of watery blood, weakness of heart, and impeded capillary circulation.

Lastly, we have said that dropsy may result from an accumulation of excrementitious materials, and especially of urine, in the blood. A case of acute renal dropsy is a type of this class of dropsies. A patient with the eruption of scarlatina fully out, or with the skin desquamating after the disappearance of the rash, is exposed to cold: the fever-poison, which was being eliminated by the skin, is thrown in upon the kidneys; these glands become congested and inflamed, the urine is scanty, high-coloured, albuminous, and often bloody, and in the course of a few hours there is general dropsy. What is the cause of dropsy in such a case?

1st. There is an accumulation of water in the blood in consequence of imperfect action of the skin and kidneys.

2nd. Urea and other urinary constituents being retained in the blood occasion an impeded circulation through the capillaries, and a consequent effusion of serum through the walls of those vessels.

One of the most convincing proofs of impeded capillary circulation consequent on contamination of the blood with urine is to be found in the hypertrophy of the left ventricle of the heart, so commonly present in cases of chronic renal disease. An analogous obstruction results from an excess of carbonic acid in the blood, as proved by the experiments of Dr. John Reid.

3rd. There is probably another case for dropsical effusion in these cases—viz., a direct irritant action of the retained urinary excreta upon the systemic capillaries; an irritant action tending to cause serous effusion, but short of that which would give rise to inflammatory exudation.

Let us now consider some of the points of agreement and of difference between *cardiac* and *renal dropsy;* in other words, between dropsy depending on a mechanical impediment to the circulation and that resulting from disease and defective action of the kidneys. In both classes of cases there is an impeded flow of blood through the capillaries, and a consequent escape of serous fluid from those vessels. In the case of cardiac dropsy the impediment is in the heart, and it acts through the veins on the blood in the capillaries. In the case of renal dropsy the impediment originates in the capillaries themselves, and is a result of the contamination of the blood with urine.

The circulation of impure blood resulting from defective action of the skin and kidneys is commonly attended with a considerable degree of febrile reaction and excitement. This form of dropsy, therefore, is sometimes called acute, active, febrile, inflammatory.

Cardiac dropsy is chronic and passive, and is unattended by febrile excitement. The blood in cardiac dropsy may have undergone little or no physical change; in cases of renal dropsy it always contains urinary excreta. The so-called inflammatory dropsy is not, however, a result of inflammation, except it be of the kidneys. The areolar tissue, which is the seat of dropsical effusion, is soft, and pits on pressure. The serous fluid is easily movable under pressure. On the other hand, inflammatory effusion into the subcutaneous tissue is fibrinous, hard, and unyielding under the pressure of the finger.

Even when the dropsy is of cardiac origin, we commonly find that there is a scanty secretion of urine consequent on the impeded circulation through the kidneys. Bear in mind that, *cæteris paribus*, the amount of urine secreted depends on the quantity of blood which circulates through the kidneys. Ligature of both renal veins causes a speedy suppression of urine; and when cardiac disease obstructs the general circulation, the urine will be scanty in proportion to the degree in which the blood is impeded in its passage through the kidneys. Thus a scanty secretion of urine is commonly the immediate precursor and cause of that which is rightly called cardiac dropsy.

Not unfrequently in advanced stages of cardiac disease, the renal congestion resulting from obstructed circulation leads to albuminuria. And, on the other hand, disease of the endocardium and valves is a frequent result of renal degeneration and consequent blood contamination. So that both the heart and the kidneys are implicated in a large proportion of cases of general dropsy.

In cases of renal dropsy, the effused fluid contains urea. In uncomplicated cardiac dropsy this is not the case.

Cardiac dropsy usually begins in the feet and ankles. The capillary obstruction is greatest in the most dependent parts, from which the blood has to ascend against gravity. The liquid, too, after it has been effused into the areolar tissue, tends to gravitate downwards.

Acute renal dropsy commonly appears simultaneously over the whole body: in the face and in the feet at the same time. The blood in these cases is contaminated with urine. There is consequently a universal capillary impediment or congestion, and an active transudation through the walls of the capillaries.

Chronic renal dropsy, on the other hand, often begins in the feet, and thence gradually extends upwards, like cardiac dropsy.

In these chronic cases the proximate cause of the dropsy is mainly the impoverished condition of the blood. The blood contains an excess of water, with a deficiency of solids, especially albumen and colouring matter. We find that patients with chronic renal disease are liable to become dropsical in proportion to the abundance of the albumen and the scantiness of the water in the urine. In cases of chronic desquamative disease, a form of renal degeneration attended with a copious secretion of urine and comparatively little albumen, there is commonly no dropsy. On the other hand, dropsy is almost invariably present in those cases of Bright's disease with the large wax-like and fat kidneys, which are associated with a scanty secretion of highly albuminous urine.

In most cases general dropsy is either *cardiac* or *renal* in its origin; but the cause of general dropsy may be in the *lungs*. Emphysema with bronchitis involves a serious impediment to the flow of blood through the lungs, and is a common cause of dropsy. In these cases an excess of carbonic acid in the blood is an additional source of impeded capillary circulation and of consequent dropsical effusion.

Let us now review the facts common to all the forms of dropsical effusion:—

1. In all forms of dropsy there is a preternatural fulness in some part or in the whole of the vascular system.

2. Whatever the cause of dropsy, the dropsical effusion is poured out through the walls of the capillaries.

3. In most forms of dropsy, and probably in all, the flow of blood through the capillaries is impeded.

The cause of the impediment differs in different cases. In one class of cases there is a distant mechanical impediment to the return of blood by the veins: pressure on a venous trunk; or disease of the heart, or lungs, or liver. In another class of cases there is an abnormal condition of the blood itself, and a consequent loss of that mutual relation between the blood and the vessels upon the maintenance of which depends the freedom of the capillary circulation.

The morbid conditions of the blood which chiefly favour the occurrence of dropsy are—1st, an excess of water, with a deficiency of solids, especially of the colouring matter and albumen; and, 2nd, the retention and accumulation of excrementitious material—most frequently urea and other urinary constituents, less commonly carbonic acid.

The *prognosis* in cases of dropsy differs extremely according to the nature of the disease of which the dropsy is a symptom. Amongst the uninitiated the belief is very general that dropsy is rarely curable; and when a patient asks concerning his malady, "Is it the dropsy?" an affirmative answer without explanation will almost invariably convey the idea of an incurable disease. The notion that dropsy is essentially incurable is happily an erroneous one. Some forms of dropsy are curable with ease and certainty. The slight anasarca which frequently accompanies chlorosis usually yields at once to appropriate treatment. Cases of acute renal dropsy, if they come early under treatment, usually end in complete recovery.

The cases in which the prognosis is most unfavourable are those in which the dropsy is dependent on advanced *chronic* disease of the kidney, and again those which are associated with organic disease of the heart or of the liver. Even in cases of incurable organic disease of the heart or kidney or liver a temporary removal of the dropsy may not uncommonly be effected.

The distress and the danger attending a dropsical accumulation are greatly influenced by the seat of the effusion. The difference in this respect is immense between the tunica vaginalis and the pericardium—between the peritoneum and the pleura (the amount of liquid being equal); and so again between the areolar tissue under the skin and that beneath the mucous membrane of the larynx.

Lastly, I will briefly indicate the principles which should guide us in the *treatment* of dropsy.

There are two objects to be aimed at in the treatment of dropsy: 1st, to remove the dropsical accumulation; and 2nd, to remove the original cause of the dropsy. If we can accomplish the second of these objects, the first is generally attained with it. The dropsy will soon disappear with the removal of its cause.

For instance, the slight anasarca which occurs in chlorotic young women is a result of a poor and watery condition of the blood. The dropsy quickly passes away when the quality of the blood is improved by nutritious food, fresh air, and exercise, with the use of iron as a tonic, and perhaps an occasional aperient.

In the treatment of acute renal dropsy, it is important to bear in mind the relation in which the dropsy stands to the renal disease; and it is especially interesting to observe the phenomena which occur during the progress of recovery. In particular, there is one phenomenon which deserves notice in connexion with the general pathology as well as the treatment of dropsy: I mean the copious flow of urine which occurs spontaneously during convalescence.

In cases of acute renal dropsy the urine is at first scanty and of morbid quality, being often high-coloured from admixture with blood, always albuminous, and usually containing numerous casts of the kidney-tubes. The scanty secretion of urine is the cause of the dropsy, and the secretion of urine is scanty because the flow of blood through the kidney is obstructed and the structure of the gland changed, the tubes being filled with desquamated epithelium and with blood and fibrin which have escaped from the gorged Malpighian vessels. Now observe what happens during the progress of cure in a case of this kind. The patient we will suppose to be placed in circumstances favourable for recovery; he is confined to bed; has a scanty diet; the loins are dry-cupped, or mustard and linseed poultices are applied there; and means are taken to excite the secretory action of the skin and bowels, and thus to lessen the work of the kidneys. Soon the secretion of urine begins to increase, until, in the course of four or five days, perhaps, the quantity of urine, which at first had been less than half the natural amount, becomes three times as great as the standard quantity, no diuretic medicine of any kind having been given.

The explanation of this spontaneous diuresis appears to be this:—During the acute stage of the renal disease, the constituents of the urine, both solids and liquids, have accumulated in the blood, and have thence been effused into the areolar tissue and into the serous cavities. Now urea itself is a most powerful diuretic; and no sooner is the inflammatory congestion of the kidney removed, and the freedom of the renal circulation restored, than the urea exerts its natural diuretic action on the kidney. The copious diuresis thus induced speedily removes the accumulated urinary solids and liquids from the blood, the areolar tissue, and the serous cavities into which they had been effused, and so the dropsy is cured.

This abundant flow of urine occurs without aid from diuretics or drugs of any kind. I have seen it occur while bread-pills alone were given as a *placebo*. Stimulating diuretics, such as squills, or cantharides, or turpentine, are injurious by increasing congestion of the kidney. The best diuretics in such cases are means which tend to lessen the congestion of the kidneys; counter-irritation over the loins, especially by dry-cupping; hot-air baths and diaphoretics, purgatives, and a scanty diet.

In some cases of chronic renal dropsy, diuretics may be given without risk, but too often without much benefit in the way of removing or lessening the dropsy. Diuretics are more frequently successful in cases of cardiac dropsy, when the kidneys are free from disease.

In the treatment of cardiac dropsy, while we endeavour to remove the fluid by diuretics and by purgatives, which excite copious watery discharges from the bowels, it is desirable to do what we can to sustain the power of the heart by nutritious food, stimulants, and tonics. Little or nothing can be done to repair a damaged valve, but much may be done to strengthen the muscular walls of the heart, and thus enable it to overcome the impediment to the flow of blood and the consequent tendency to dropsy which a diseased valve occasions. When other means fail to remove a dropsical accumulation, we may often afford great temporary relief, and prolong life sometimes for a considerable period, by mechanical means—by tapping the abdomen, for instance, in a case of ascites; by acupuncture or incisions through the skin of the legs for the removal of anasarca.

It is very interesting to note the phenomena which follow upon puncturing or incising the legs in cases of anasarca. There is first a copious drain of liquid from the punctures. Secondly, there is a further exudation of liquid from the over-distended bloodvessels; this liquid also escapes from the punctures or incisions, and its escape is often associated with temporary symptoms of exhaustion, such as a rapid and feeble pulse and pallor of countenance. Thirdly, there occurs often a copious secretion of urine, in consequence of a more free circulation of blood through the kidneys.

Dropsical accumulation tends to cause a secondary impediment to the circulation by the pressure of the effused liquid from without upon the small bloodvessels. And, again, the capillary circulation becomes more and more impeded in proportion to the increasing distension of the bloodvessels which results from cardiac or renal disease. The drain of liquid from the areolar tissue, allowing of a further exudation from the bloodvessels, thus removes or lessens the obstruction which results from over-fulness of the vessels. The general circulation therefore becomes more free, and the greater freedom of the circulation through the kidney is attended, as we have before seen, by a more copious secretion of urine.

The free action of a hydragogue—elaterium for instance—is often followed by a copious secretion of urine. The gorged vessels are partly unloaded by the drain of liquid from the bowels; the circulation through the kidneys, as through other parts and organs, consequently becomes more free; and hence a copious secretion of urine, and a rapid diminution or even a complete removal of the dropsy.—*Lancet, Jan.* 16, 1864, *p.* 59.

7.—ON TRICHINA SPIRALIS.

By Dr. W. Müller, Homburg.

For many years past the Trichina spiralis (class Nematodes) have been found in the muscles of hogs, wound up in a spiral form, and enclosed in chalky capsules; and Dr. Owen discovered and described them in the muscles of men. Professor Zenker, of Dresden, was the first who proved, by a full and exact statement of a case, and a careful and minute post-mortem examination, that the development and wandering of the trichinæ in the human body produce violent symptoms similar to those of typhus fever, and cause, most probably, in many cases, the death of the individual.

Experiments made by Professor Virchow, of Berlin, and Professor Leuckart, of Giessen, by feeding animals with pork in which were trichinæ, proved the same as Professor Zenker's observations in the above-mentioned case—namely, that the trichinæ, when taken into the stomach, commence almost immediately their development, male and female, and innumerable embryons. The young worms, perforating the intestines, enter the muscles, and, wandering in them, produce the violent symptoms of the disease above-mentioned, until they become incapsulated in the muscles, in which state they are innocuous.

In Hettstaedt, a small town in Prussia, containing about 5000 or 6000 inhabitants, a veritable epidemical propagation of trichinæ commenced in the middle of October last, in consequence of the infected persons having eaten a kind of sausage (not thoroughly cooked) made of pork in which were trichinæ.

In some cases small portions of muscles were taken by Middeldorpf's harpoon from the persons infected, and whilst suffering with the disease; and by submitting those portions to microscopical examination trichinæ were discovered in them.

In the evening of the 9th of November last I was summoned by a telegram from the physician of Hettstaedt, informing me that a relation of mine was suffering from the trichina disease; that he had also a pneumonic affection, and was very ill. On my arrival on the following day I found the patient—who previous to the attack was a strong and very healthy man, twenty-three years of age—perfectly conscious, with a slight œdematous swelling of the face. On examination of the chest, a dull sound over about an inch and a half of the lowest part of the lower lobe of the left lung was produced by percussion; crepitating rattles were audible, but there was no bronchial breathing, thus showing the beginning of resolution of the pneumonia; at the lowest part emitting the dull sound there was a slight pleuritic rubbing. The pulse was 140; respirations 48; and the temperature of the body 39° centigrade.

The symptoms of the disease commenced on the 16th of October with loss of appetite and diarrhœa, followed by a sensation of painful weakness in the limbs and difficulty in moving the tongue; the pulse being above 100. The patient was not confined to his bed during the day-time until the 6th of November, when the pneumonic symptoms commenced.

The day after my arrival (Nov. 11th) the pneumonic symptoms were unaltered, with the exception of the pleuritic rubbing, which had moved a little higher up. The whole of the pleuro-pneumonic affection was so very trifling that it certainly did not account for a pulse of from 140 to 150, and for the violent oppression, or rather, as the patient explained it himself, "the weakness in drawing his breath."

The following day the frequency of respiration varied between 30 and 60; the pulse was more than 200, and very weak; the temperature had fallen to 38° 6′ centigrade; and the body was covered with a profuse clammy perspiration. The other physical symptoms were the same as before, and the pleuritis had not extended higher. The complaint of weakness in breathing, or, as the patient called it, "the impossibility of drawing a sufficient quantity of air into the lungs," was increased; but he remained conscious and resigned, so much so that he several times asked me at what hour I expected he would die.

At seven o'clock on the evening of the 12th of November he died.

The post-mortem examination, performed on the 13th, proved an infiltration of a part of the lower lobe of the left lung, extending upwards about an inch and a half from the lower margin of the lung, and about three or four ounces of liquid exudation in the pleural cavity of the same side. When examining the chest and intercostal muscles, I found, in every small piece of the muscle placed under the microscope, trichinæ partly wound up, but not capsulated, partly forming a single sling, and partly extended. In the examined parts of the heart and diaphragm no trichinæ were discovered.

On the day previous to the above-mentioned post-mortem examination I examined with the miscroscope several small pieces of muscles, which had been taken from the bodies of persons who had died of the disease, and were given to me by the physician of Hettstaedt, Dr. Rupprecht, and I found a considerable number of trichinæ in them.

Previous to my departure from Hettstaedt, eighteen to twenty persons had died of the trichina disease, and more than eighty persons were at that period afflicted with the same malady, produced by the same cause.

According to the information I obtained on the spot, the disease begins, a few days after eating the meat in which there were trichinæ, with loss of appetite, and almost without exception with diarrhœa and fever; œdema of the eyelids; also pain, or at least painful sensation of weakness in the limbs; œdema of the joints; difficulty in moving the tongue; profuse clammy perspiration: and those patients who do not become convalescent, die either unconscious with symptoms of typhus fever, or, in a few cases, remain conscious to the end, complaining of inability to breathe freely.

The only important symptom of typhus absent in the disease is the enlargement of the spleen, and it is very probable that some of the so-called epidemics of typhus fever in former days were caused by the propagation of trichinæ in the human body.

Since the disease has been known (about three years ago) a great many cases have been observed in Germany.

The vitality of the trichinæ is not destroyed unless the meat or other substances in which they are located be subjected to the temperature of boiling water for a sufficient time to ensure that every particle has been acted upon by that degree of heat. Salting and smoking trichinous meat, as is usually done, does not appear to be sufficient to destroy the worms in all parts of the meat.

Picric acid (acidum picro nitricum) was tried with the hope that it might be administered with success to the patient, but it failed.

In trichinous pork of a pig killed with picrin acid, the worms were found alive.—*Lancet, Jan.* 23, 1864, *p.* 93.

8.—TWO CASES OF SMALL-POX SUCCESSFULLY TREATED BY SARRACENIA PURPUREA.

By Dr. Wm. Henderson Grant, Aberdeen.

[We always consider the action of medicines an interesting topic, and consequently generally extract from the medical journals any information likely to be of use on this point, but we consider the action of such a remedy as the American pitcher plant peculiarly liable to be overrated. We have ourselves tried it, but we cannot say that the cases in which it was administered ran any different course from those in which nothing at all was given. In several, the eruption rapidly died and disappeared without leaving any pitting, but the same happens in many cases in which no medicine has been given. The question can only be determined by a very extensive and impartial trial. Of the following two cases, one was treated by Dr. Grant's brother.]

I quote extracts from two letters from my brother:—"I am giving a small-pox patient the sarracenia purpurea, in infusion; I commenced it yesterday. The eruption appeared four days ago, and is very abundant—a sort of semiconfluent variety, not *very* serious, but not by any means slight. In answer to my inquiry to-day, how he felt (I was anxious to know the *immediate* effects, if any, of the sarracenia), he said he was quite well in health. He is placed in very unfavourable circumstances—a miserable little place of an attic inhabited by three persons." In a note that I received a day or two later he says: "My small-pox case has quite recovered. The sarracenia seems to have some specific action, although most M.D.s say it is a hoax. I gave it to this man upon the fourth day of the eruption, up to which time the disease had progressed exactly in the ordinary manner, the vesicles containing clear lymph; but here they were arrested, they never went on to suppuration, and there was no odour perceptible. Prior to his getting the infusion he complained much of restlessness and sleeplessness at night, but after he commenced taking it he slept well. On my last visit, instead of finding symptoms of feverishness, I found him sitting by the fireside, telling me that he 'was quite well, but just not so strong.'"

To give the particulars of my own case would be to repeat a good deal of the substance of the above. The patient, aged twenty-five, was a man of a very full, plethoric habit. The symptoms were severe, and the eruption very abundant, quite of the confluent form on the face; the mouth and fauces were also covered. To extreme restlessness and sleeplessness at night was added pretty smart delirium. The patient commenced taking the infusion on the afternoon of the fifth or sixth day. Relief was almost instantaneous; he slept soundly during the succeeding night, and had no more delirium. When I inquired for him next day, he replied that "he had felt quite easy since I gave him that *stuff*," indicating the infusion. He continued rapidly to improve, and is now recovered.—*Lancet*, *Feb.* 6, 1864, *p*. 161.

9.—ON THE EFFICACY OF SARRACENIA PURPUREA IN ARRESTING THE PROGRESS OF SMALL-POX.

By Dr. J. Taylor, Old Kent-road, London.

[Dr. Taylor has employed this remedy very extensively, and considers that its utility is unmistakeable.]

The following is the formula employed:—Two ounces of the sliced root; three pints of water, boiled in a closely covered vessel, down to two pints, and strained. The first case was a little girl, six years of age. She was seen on the third day of eruption, of primary small-pox, and immediately began to take the decoction—four ounces per diem in divided doses; and in less than twenty-four hours the mother reported "she had been better ever since she began to take the medicine." The eruption was very extensive, pustules large, and in some places confluent. The case advanced apparently without interruption until the seventh day, when the pustules began to shrivel, and on the eleventh day the desiccated scales had nearly all fallen off; no pitting; patient convalescent. Every succeeding case was treated in the same manner and with the same success. Only two cases perished: one an infant three weeks old, who took it from its mother; the other an adult female, who seemed to sink from pyæmia on the third day of the eruption.

One remarkable fact, however, deserves to be recorded. A poor woman brought her child, a year and a half old, to my surgery one night at eleven o'clock, with the eruption of small-pox, which had on that day made its first appearance. I gave her the decoction, with directions to give a dessert-spoonful four times a day. On the fourth and eighth days her visits were repeated, each time speaking in laudatory terms of the efficacy of the medicine. On the eleventh day of the eruption she made her last visit, making at the same time the following pertinent remarks: "I have called to thank you, sir, for your great kindness. My child is now quite well, all the scabs have fallen off, and the skin has not a blemish on it. That medicine must be very valuable, sir; for a little girl, the daughter of one of my lodgers, caught the small-pox three days before mine did, and it has not changed a bit; the pock is all over the body yet in great mattery heads, and here is my child with her skin nearly as clean as when she was born." I was somewhat incredulous as to this perfect clearance of the skin; but in half an hour she brought the child for my inspection, and the appearance fully corroborated all she had said. She had not been vaccinated.

The decoction requires to be carefully prepared; the root should be thinly sliced, and boiled in a closely covered vessel, and for adults the quantity of the root should be increased to three or four ounces to the three pints of water, and about eight ounces administered in the twenty-four hours in four doses. It agrees well with the stomach, possesses eliminative properties, being decidedly diuretic, and generally maintains a daily action on the bowels. In a few cases while under the influence of the remedy, the eruption assumed a papulous character, and terminated in the usual way by desiccation. There is another specimen of the drug in the market, which is composed almost entirely of the pitcher-formed leaves; this is greatly inferior to the root, in fact is almost inert, and in my opinion deserves to be discarded.

Apropos of external applications for the prevention of pitting, I have not found anything to answer better than the old-fashioned tripe-water, prepared by boiling fat tripe in water, straining the liquor, and applying it warm as a lotion or a fomentation to the face several times a day; it affords great relief to the distended pustules, which by their backward pressure play an important part in the production of pitting.—*Lancet, Dec.* 5, 1863, *p.* 664.

10.—ON SARRACENIA PURPUREA.

[The following is the opinion of Dr. James Watson on the action of sarracenia purpurea. The cases occurred in the Edinburgh Royal Infirmary.]

In one of the small-pox wards under Dr. Haldane's charge, the sarracenia purpurea in infusion was administered to eight patients, the effect carefully watched in each, and the result in all was that the disease ran its natural course. The sarracenia purpurea was found absolutely inert; it produced no effect whatever.

[Dr. Watson then reports one case which he especially watched, and observes :]

In this case, it is most certain that the sarracenia purpurea did not in the slightest shrivel or wither up the eruption, nor did it seem to affect the patient in any way. It has the virtue, which happily many—unfortunately not all—new drugs possess, of being perfectly innocuous. So much and no more can we say in its favour.—*Edinburgh Medical Journal, Jan.* 1864, *p.* 623.

DISEASES OF THE NERVOUS SYSTEM.

11.—ON DELIRIUM TREMENS, AS DISTINGUISHED FROM OTHER EFFECTS ON THE MIND, REAL OR APPARENT, OF EXCESS IN DRINKING.

By Dr. William Seller, F.R.S.E., Fellow of the Royal College of Physicians of Edinburgh.

[Pains should always be taken to distinguish delirium tremens from other mental disorders, which either do or seem to take their origin from excessive drinking, for one may very readily be deceived in regard to the maladies in question by dwelling oo much on their apparent cause.]

To pass over irregular forms of fever and inflammatory affections of the brain and its membranes which may succeed drinking and sometimes cause difficulty, the other resembling maladies, to which the name of delirium tremens should be refused, come, as far as my observation has extended, under the three following heads :—First, Abortive cases or threatenings of delirium tremens; Second, Cases of insanity, either temporary or more permanent, the immediate precursor of which is excessive drinking; Third, Mere fits of drunkenness, with much violence or extravagance, but without distinct delusion.

Abortive Cases.—What I term abortive cases are often alarming enough, as presenting even all the symptoms usually premonitory of the perfect disease, while their progress only stops short of what must be regarded as the essential characteristic of a regular form of delirium tremens, namely, a delusion or succession of delusions commanding belief and leading to action.

In abortive cases, there may be a greater or less change on the patient's ordinary bearing; there may be alteration of the voice; there may be a sudden starting in the midst of a sentence, and a fixing of the attention on something at a distance in the room; there may be even tremors of the hands, tremors and startings of the lower extremities, and a tremulous tongue, while such alarming symptoms may have been preceded by nearly sleepless nights and uneasy dreams; moreover, these dreams may be attended with a momentary belief in their reality after the patient awakes. If, however, he can dismiss the illusions of his dreams, he may fall, either with or without the aid of treatment, into a quiet sleep and awake nearly recovered. Sometimes the abortive case takes on a more chronic character, and may continue for a number of days, the patient sleeping heavily at intervals, awaking to long fits of restlessness, all the while being averse to be left alone, and troubled with sudden severe attacks of vomiting; yet, at length, he begins to obtain refreshing sleep, and gradually gets well. The chief obstacle to the immediate restoration of health in most seizures of the abortive disease is the derangement of the digestive organs which is so often concomitant.

Cases of this kind are unquestionably threatenings of delirium tremens, but being destitute of the characteristic irresistible delusion should be kept apart and denied that name. To draw inferences from such abortive cases, and to make these inferences a part of the medical history of the perfect form of the disease, can lead, it is plain, to nothing but error.

Every medical practitioner must be familiar with abortive cases of this kind, and to designate such delirium tremens is neither conducive to the improvement of medical knowledge, nor just to the patient, who is sometimes far from being an habitual drunkard—an imputation implied in the having undergone an attack of delirium tremens. It happens that men entitled to be described, at least, as not intemperate, are sometimes so unwise as much to exceed their usual allowance of liquor, when things occur to create great mental annoyance. In such circumstances, I have several times seen symptoms arise of the kind just referred to. Here, probably, the mental affection which led to the excess, is as much concerned in the origin of the disturbance as the excess itself.

Of the effect of mere harass of mind, aggravated by the loss sleep, to produce transitory delirium in a person not constitutionally disposed to mental derangement, I had a striking example during the great railway mania, some fifteen or sixteen years ago. I was sent for to the counting-house of an Edinburgh firm about mid-day, where I found a gentleman from the country held down on the floor by several people. He was struggling violently with them, and by turns raving on the idea that he was charged with having committed some great offence. I had persuaded the people struggling with him to allow him to get up, when, just as he rose to his full height, and he was particularly tall, the door opened, and an eminent medical man, who also had been sent for, entered. Towards him he immediately rushed with his uplifted fist, calling on him by name, in a loud voice, to say if he was mad. My friend, though the very reverse of a timid man, was naturally alarmed, and felt strongly inclined to send the patient at once to an asylum,—but it was finally settled first to make trial of treatment, for a few hours, as there was a bed available, away from the bustle of business, at the top of the house. After an opiate he had some sleep, and in the evening, though still wavering, he was plainly much more himself. Next morning, after a tolerable night, he had so much recovered, that there was little doubt of the paroxysm passing entirely away. He came round quickly, and in the long period which has intervened, he has never shown any tendency to mental disturbance, but has been, what he always was esteemed before, an active and very efficient man of business.

Previously to the attack, which commenced in the night before, he had been for some weeks overwhelmed with occupation, and, finally, had fallen away from his sleep. The circumstance, I believe, which his troubled imagination had converted into a crime, or at least into a grave offence, was, that being arbitrator in a matter of importance, he came to think that he had allowed some evidence to be recorded in behalf of one of the parties which ought to have been rejected. It was certain that this gentleman had been absolutely temperate for some time before. If it had appeared that he had taken an extra glass, for a few evenings before the attack, in order to calm his mind and procure sleep, how readily might this case have figured as one of delirium tremens, cured by opium!

Cases of Insanity, temporary or more permanent, the immediate precursor of which is Excessive Drinking.—It not unfrequently happens that an outbreak of insanity closely coincides in point of time with an excess in drinking. I do not here refer to the form of insanity which has been termed oinomania, or, somewhat absurdly, dipsomania. If there be just ground for establishing oinomania as a form of mental derangement, the insanity should precede, not follow, the intemperance. The real insanity, in such a case, is the propensity, the uncontrollable desire to indulge in drinking at the cost of everything which is most prized in social life. It is still doubtful if oinomania be a well-founded distinction. As men are subject to vicious indulgence in tobacco, opium, and other narcotics, so are many prone to periodical fits of drunkenness, who are mad neither when they begin, nor during the course of the paroxysm of drunkenness. Such men often sacrifice everything to this inordinate propensity. The paroxysm is called oinomania only if they show signs of madness during the fit. Thus, the madness, when it occurs, is not the cause of the drinking, but the effect of the excess.

Among persons at large who are either on the verge of insanity, or actually insane, the abuse of strong liquors may give rise to cases apt to be confounded with delirium tremens. As delirium tremens has very much the character of a specific disease, it should not be rashly assumed that such persons are even susceptible of this malady, or that delirium tremens having arisen in them may be prolonged into acute mania or some other form of mental derangement. For, in the meantime, it seems preferable to keep all cases in which a taint of actual insanity is discoverable apart for further investigation, and to regard delirium tremens in its regular type as a disease brought on in the perfectly sane by excessive drinking.

Persons at large with a taint of insanity may be either under what is called the incubation of insanity, that is under a growing demonstrative predisposition to the malady which will at last break out, or may be at all times constitutionally liable to become insane under the application of certain definite exciting causes, or may be actually insane, yet possessed of a power to control the manifestation of their condition, unless when the sudden occurrence of new circumstances deprives them of such control and renders their state of mind apparent to the world.

The phrase "incubation of insanity" is familiar to medical minds. When an incubation, which plainly is but another word for an accumulating predisposition not unmarked by discoverable signs, has attained a sufficient development, any strong excitement that happens to occur may prove the immediate cause of the outbreak of the disease already impending. Nothing then is so likely to be followed by such an effect as deep excess in drinking. If the medical attendant be not made aware of the evidence of a previous incubation of insanity, he can hardly avoid ascribing the outbreak to delirium tremens. It may even happen that, during the incubation of insanity, several successive transitory outbreaks of delirium may succeed as many occasions of excess, before the final, more permanent malady becomes established. Observations made on such outbreaks seriously mislead, if used to illustrate the character of genuine delirium tremens.

The following case exemplifies the occurrence of an attack resembling delirium tremens during the incubation of insanity. Though but one outbreak was observed in this patient it is easy to believe that several transitory outbreaks might have occurred, under the like circumstances, at an earlier and less developed period of the incubation.

A medical student of twenty-two or twenty-three years of age came to Edinburgh to finish the studies which he had pursued for some time at Paris. He was from the first observed to be highly eccentric in various particulars, singular in his sentiments and manner, and, above all, very suspicious and jealous of being slighted by those about him. He was habitually temperate and regular in his conduct. Being, however, of a hospitable turn, he was accustomed to give an entertainment in a tavern to his intimate companions as often as he received his quarterly allowance. On one occasion this entertainment was prolonged to a very late hour; the next morning he was found to be very noisy and delirious. One of his illusions was, that some young men unfriendly to him had employed little boys to shout out, in front of the house where he lived, that he had not paid the last night's tavern bill. The disease, owing to the mode of its origin, the aspect of the patient, and the kind of delirium, was naturally regarded as allied to delirium tremens; nevertheless, it turned out to be a case of acute mania, or, at least, after a few days it took on that character. Without having entirely recovered from mental derangement, he died within a year of rapidly-developed phthisis.

During the incubation of insanity it is sometimes discovered that persons distinguished previously for temperate habits had frequently indulged in secret drinking. The drinking in such a case is not always related to oinomania. There appears to be no irresistible impulse to excess,—it is simply one of the irregularities of conduct developed by the general failure of self-control. Yet if an outbreak of insanity take place coincidently with one of these indulgences, the whole disease may for a time be imputed to excess in liquor. This at least may occur in the absence, as so often happens, of exact accounts as to the previous condition of the patient.

A gentleman, remarkable among his intimates for habitual restraint in the use of wine, was discovered to have generally consumed a bottle of wine during the night for a considerable period, during the incubation of mental derangement. This excess, though it may have hastened the progress of the disease, did not appear to have any particular connexion with its actual onset; neither did the desire for intoxicating liquors take any prominence during the continuance of the malady, which was prolonged for several years.

Our next subdivision includes the cases of persons at all times constitutionally liable to become insane under the application of certain definite exciting causes. It is commonly received that the victims of this unhappy predisposition suffer from the remains of some disease of the brain or injury of the head in early life. When such persons cannot resist the temptation to drink to excess, their lot is very miserable. They commonly recover at once when put in confinement, and often must suffer all the irksomeness of asylum residence for life without a remedy. While at large they are seldom free from singularities of conduct sufficient to attract attention. Some of them, in popular language, are described as deficient so many pence in the shilling. They are frequently conscious of their deficiencies, and make unavailing efforts to remedy them. Some are quite capable of business; others are unfit for continued application to any pursuit. It is common to the persons now referred to that in general they conduct themselves with sufficient propriety. But with a certain number it happens that when they are led to indulge in liquor they become quite maniacal, often violent in the extreme, and very prone to do injury to others. It cannot be doubted that many in this condition being apprehended by the authorities and sent to an hospital, serve to swell the list of cases of delirium tremens spontaneously cured.

A young man, on whose early education every pains had been bestowed, was discovered to be eccentric in various particulars. As he approached manhood he failed in every attempt made by his relations to put him in the way of fitting himself for a profession or occupation of some kind. He was supposed to have suffered in infancy from disease of the head. He showed no defect of intelligence in ordinary conversation, being able most commonly to maintain his part, even in argument, without betraying any signs of ignorance, or deficiency of understanding. He was, however, manifestly dispirited by the consciousness of the presence of some mental defect. Latterly, he came to indulge at times in drinking, and then he became furious, threatening his relatives in the most alarming manner, and sometimes even attacking strangers. He was repeatedly sent to an asylum, for such violence, where, after a day or two, he regained his former state of mind. At one time he seemed to improve very much, so that a hope arose that the disease was passing away. But the same violence on a return to drinking has recurred.

I was, not long ago, asked to visit a case of this kind in the Morningside Asylum, on the part of the parish to which the patient belonged. He had been sent there by the authorities of a neighbouring shire on account of a murderous attack on relatives while under the influence of liquor. I found a young man as quiet and conversable as could well be described, actively engaged in assisting at some out-of-door work. He had shown no sign of derangement in the asylum; but the charges against him on several occasions before he was sent there, when excited by liquor, were too definite to permit me to recommend his dismissal, unless some friend would be surety that he would keep him from drinking.

Our next subdivision of persons at large with a taint of insanity includes those who are actually insane, but possessed of a power to control the manifestation of their condition, unless when the sudden occurrence of new circumstances deprive them of self-control and renders their state of mind apparent to the world.

It cannot be doubted that there are at all times not a few persons at large who are inwardly in a state of insanity, who indulge within themselves in morbid imaginings, viewed as realities, who are, nevertheless, able for the most part to suppress, by the power of the will, any considerable manifestation of their disorder. If men so affected ever yield to the temptation of drinking, it is readily to be believed that their self-restraint will be lost, and the usual indications of insanity brought forth. As soon as the immediate effects of the intemperance pass away, most commonly not till several days have elapsed, the control of self returns, and the disease seems to be cured. I have had an opportunity of becoming acquainted with an instance, which I believe to be of this nature, where successive outbreaks, at considerable intervals, were mistaken for attacks of delirium tremens.

Though it is foreign to our present purpose, I will add an observation which I think deserves consideration. In the kind of suppressed insanity just referred to, the outbreak may occur not only from a fit of intemperance, but from any sudden, real, or supposed provocation, while the violence so excited may even be homicidal. Thus a madman at large able to suppress the indications of his disorder may, without apparent cause, but from some concealed motive, break from his self-restraint and commit a crime. I do not propose this as a theory to account for what is termed impulsive homicidal tendency, yet it is not unlikely that some of the recorded cases of homicidal insanity on apparently sudden impulse come under this description.

The power of suppressing the indications of mental derangement which belongs to some insane persons deserves a closer examination than it seems to have yet received. With a power of this kind exerted for a short time, as, for example, during examination by a physician, the object of which is known to the patient to be confinement, the medical profession are sufficiently familiar. It less frequently happens that full evidence is obtained of a long-continued self-restraint, such as is indicated above. In the view I take I rely much on the latter of the two following cases: both lie within my own experience.

A well-educated young man, affected but for a few days with acute mania, was admitted into an asylum, where he immediately became to all appearance well. He was made useful in the house, and in a short time was sent out on excursions in charge of patients, and was even permitted to take them to amusements in the town. At the end of three months he was dismissed cured. He went back to the house from which he was taken three months before, and in less than two hours he was as ill as on the first occasion. He was admitted again to the asylum, and immediately seemed quite well. At the end of other three months he was again dismissed, and travelled for a while before returning home. This happened a number of years ago, and there has been no indication of derangement since.

A gentleman of middle age was attacked with insanity, accompanied with much extravagance of speech and sentiment. After some weeks it was proposed to send him to an asylum, and two medical friends visited him one day in order to draw up a certificate. That day, for the first time after the outbreak, he suppressed all extravagance, and spoke quite rationally. From that day for several years during which he lived, he betrayed no distinct manifestation of derangement; yet his original character was much changed, and it was the conviction of those who enjoyed his intimacy that he cherished some of his original delusions to the last.

Fits of Drunkenness, with much Violence or Extravagance, but without Distinct Delusion.—Some men drink daily to excess without ever showing any violent excitement, even that of intoxication, till, their constitutions being gradually broken, they fall into a premature grave. Of this description not a few are attacked by delirium tremens, and in these its complications are numerous, while the disease is proportionately dangerous.

Others, again, are subject to fits of drinking. They go on the *spree*, to use a vulgar expression, sometimes for a day or two, sometimes for a week or fortnight, or even for a longer period, and then abstain, it may be, altogether, for a longer or shorter time. In this way a man live may for a good many years if the intervals of sobriety be considerable; and at times these extend to several months. But this drunken paroxysm may readily be confounded with delirium tremens, particularly if the patient, being of the labouring order, and less trained to carry his liquor decently before the public, be laid hold of by the police, and sent to an hospital. Such a paroxysm may continue for a fornight or more without any illusion or any symptom which entitles the drunkard to that immunity for crimes which is now conceded to the victim of delirium tremens.

A man under this drunken paroxysm is often guilty of great violence, yet that violence is under a kind of restraint. He breaks furniture, turns his wife and children out of doors, and commits every extravagance and absurdity within his own house. That he does not often fall into the hands of the police, if his station be above that of the labouring class, is only the stronger proof of his being able and willing to exert control, unless where he feels himself to be lord and master, as in his own house.

The quantity of spirits consumed in a prolonged paroxysm of this sort, especially when money is not wanting is wholly incredible. It dos not, in general, produce the ordinary symptoms of intoxication, or at least these symptoms, if produced at first or occasionally, pass away during sleep, and there remains a restless active state of mind, kept up by the repetition of the stimulant at short intervals. The craving for this continued supply of spirits is now irresistible, and the least attempt to interfere with its gratification provokes the utmost violence of temper, if the persons around be under the control of the drunkard, as members of his family and the like. If those beside him be not likely to be influenced by threats and violence to connive at the excesses he is practising, all manner of cunning is developed, most commonly with falsehood, to an inconceivable extent;—for there is this in common to the drunken paroxysm with insanity, that, even in persons whose ordinary sentiments in health are honest and upright, there is no reliance to be placed on what they say; and this strikes us the more as being necessarily oftener observed in the female sex when under the influence of this dreadful malady.

Even when no opposition is offered to his will, the drunkard often displays the utmost violence of temper; nothing can please him, he pours forth volleys of abuse on his children who are exerting their utmost to gratify his wants. The secret of this last kind of violence is manifestly his dissatisfaction with himself; he feels conscious of his misconduct and of the injury he is doing to his own health, and insensately pours forth the ill-humour he is in with himself upon the members of his family.

When the quantity of spirits taken daily is great, and the paroxysm much prolonged, the drunkard can neither be advanced in life nor weak in the digestive organs. The youngest and the strongest must at last yield to the power of the stimulant. Sickness at stomach begins to prevail, and, finally, severe fits of vomiting occur, of greater or less duration; after which, the drunkard falls into a long sleep, and often awakes next morning fit for his usual employment. The periodic character of the vice may be retained for a long time; for example, for twelve or fourteen years. Most commonly, however, after a time the intervals become shorter and shorter, and the mode of the drunkenness changes to that of daily indulgence, to such excess as, if not to unfit the victim for business, at least to undermine his health and lead to a premature death.

It would be surprising if one description, such as that just given, could include all cases of the same unhappy character. There are many varieties depending on the age, sex, position in society, and the personal peculiarities of the victim. Nevertheless, there are many points in common, and, in particular, there is the negative diagnostic sign, namely, the absence of delusions, such as compel belief and lead to action.

In a case of this kind, of a very extreme description, which was under my observation for more than fifteen years, till he was cut off by a chronic malady, the patient knew well that he exposed himself each time to the risk of an accession of delirium tremens, yet in all that period he never felt its approach but once; he took alarm when he became sensible of illusions which he could not repress, and instead of leaving his family, as usually happened, to send for the doctor, he ran to seek him himself. Being got home to bed he was soothed to sleep without opium, by continual assurances that nothing preternatural was in the room; and the disease made no further progress.

It is not to be denied that an impairment of the power of self-restraint is common to every form of considerable intoxication. But many facts show how far it is from being wholly lost, and how far it is possible for the rules of society to compel its cultivation. The impairment of self-restraint is seen remarkably exemplified in the intoxication of very young men not yet trained to manners, and in that of men in the lower orders, on whose minds the necessity of orderly conduct has not been authoritatively impressed. With this latter fact, the observation of the late Duke of Wellington remarkably coincides, namely, that officers raised from the ranks often fail to carry their liquor like gentlemen. On the other hand, it has been a frequent subject of remark, that a person already beginning to talk extravagantly under the influence of liquor is restored to sobriety by the entrance of a stranger or of one in whose opinion he is desirous to stand well. Innumerable similar evidences will occur to every one to the same effect, or of the power of self-restraint which can be exerted even in deep intoxication. It is true that even in delirium tremens the patient may sometimes be reasoned out of his follies; but this is only for a time, the delusion which exists soon resumes its empire, and then all his extravagances recur as before. Till delirium tremens is established, the effects of drink are very different from madness. The notion that a man simply under the influence of strong liquor has no power of self-control, should receive no countenance.

The incident I am going to relate illustrates the kind of double self which is often seen active in the prolonged paroxysm of drunkenness. A man engaged in an extensive business was addicted to long fits of drinking, with considerable intervals of complete abstinence. During one of those fits he got into a quarrel with a neighbour, owing to the intolerable noise he was making far on in the night, and the police had interfered. His family, thinking to avoid exposure, sent to my house, which was near, for aid. Having got him into his own house I thought to persuade him to go to bed. He said that owing to having to go out at six o'clock it was not worth while to go to bed, as it was now past four o'clock. I rashly said that I would not go away unless he went to bed. But he remained determined, as, he said, he had to set men to some new work at six o'clock. Finding I was resolved to stay, he began to talk on common topics, and we got into an animated discussion on Palmer's trial, then recently over. During this time he asked for no more liquor. When it was near six o'clock he started from his chair and went to another part of the house. I heard him shouting loudly for something he wanted, while nobody answered. I then heard a loud crash, and going out I met him brandishing a heavy carpenter's axe, with which he had just split the top of a mahogany table into two; he seemed much excited, and coming towards me with axe uplifted, he said, "What's to hinder me?" I said, "No; you know that I never come here but for your good." "I dare say you are right," he said in a subdued tone, laying down the axe. We went out together. He walked steadily, but with a kind of effort, his eyes on the ground. The paroxysm continued for a few days more, the consumption of spirits, especially in the night, being enormous; finally, as usual, vomiting came on, and, after a long sleep, he recovered his natural character, becoming in the highest degree penitent, yet at that time hopeless of reformation.

Delirium tremens presents so great a similarity in its course, under the most opposite circumstances, as to approach to the character of a specific disease, like small-pox or measles, in the train of symptoms exhibited. The description of this disease which best accords with what I have myself witnessed, is contained in the memoir by Dr. Ware, of Boston. His account extends beyond what I can vouch for, namely, to the history of the malady when left to run its course without treatment. In the following condensed view I rely on his authority particularly for nearly all that belongs to that aspect of the disease.

The premonitory signs are not always present. These are tremors of the hands and of the lower extremities, and, in particular, of the tongue; tremulousness of the voice, with indistinct articulation; a hurried manner, general anxiety; startings and twitches of the limbs. These premonitory signs, if not peculiar to such instances as are conjoined with great disturbance of the digestive organs, are at least seldom absent in cases of that kind. Whether such signs precede the disease, or whether it developes itself of a sudden, the subsequent course is little interfered with. When the patient is under observation from the first, it is found that for one or two nights before the delirium begins, he sleeps ill, is troubled with uneasy dreams; he even gets up and wanders through his house. The dreams assume more and more the character of realities; but so long as the patient can dismiss the visions of his sleep as unsubstantial, it seems possible to check the disease. This seems to be sometimes accomplished, both at this stage and at the earlier stage, when the occasional premonitory symptoms above enumerated occur. Persons who have been several times affected with delirium tremens feel themselves safe so long as, on waking, they can withhold belief in their dreams, however vivid. Sometimes there is a night or two of complete watchfulness before the delirium is apparent, yet very often the delirium begins by an impossibility of shaking off belief in the reality of the last dreams. On rare occasions the medical attendant is the first to detect the delirium; more frequently it has been already remarked by the family, and is not exhibited at the medical man's subsequent visit, particularly if he is a stranger, so that the patient puts himself more on his guard. Nevertheless, when no actual delirium is detected, the natural manner of the patient is changed, something will seem suddenly to catch his attention, towards which his eye is all at once intently directed. His voice is altered; he speaks louder, or with more haste than usual; he starts from bed or from his seat, and attempts to gather something from the carpet, or looks under the sofa or the bed, as if he imagined something were there; he will even insist on ripping open the bolster or the pillows to get rid of some animal lurking there. If such symptoms occur in the early part of the day there is usually distinct delirium established before evening.

There can be no doubt, I think, that if sleep can be procured even during the first night after the delirium has been thus completely established, the disease, as far as the delirium is concerned, is either removed or materially shortened.

But when the disease is left to itself during the first night of complete delirium (and that I have had occasion to remark), there is entire watchfulness, and the delirium increases during the first hours, and declines somewhat towards morning. In the morning it continues unaltered in character, but the patient is more tractable than during the night. The second night is generally worse than the first, and though there is some alleviation towards morning, yet in the early part of the third day the patient is worse than on the second day; but when the disease is to have a favourable termination the delirium of the third night is less violent than that of the preceding. The disease then, it appears, terminates spontaneously, either on the evening of the third day, or in the early part of the night, but more commonly not till the latter part of the night, or the morning of the fourth day. In those who have had repeated attacks the malady is apt to be protracted longer, even when the event is to be favourable. When the attack is to terminate unfavourably the delirium continues undiminished till the fatal event takes place.

The delirium in this disease is not often violent. The strange vividness of the illusions seems to impress the patient with a kind of awe. Nevertheless he is sometimes very noisy, and utters loud shrieks. These, I have thought, to result from an aggravated anguish of mind, founded on mental distress existing prior to the excess in drinking. The delirium has a more coherent character than that of fever. The patient can sometimes be reasoned out of his delusion for a while, but it quickly recurs.

Too much attention cannot be bestowed on the exact character of the delirium in every case regarded as belonging to this disease, for as often as the delirium does not square with the received standard, or any deviation otherwise takes place from the ordinary type of the disease, a new diagnosis should be instituted in the well-grounded fear that what has to be dealt with is not delirium tremens, but some other of the mental disturbances connected with excessive drinking.—*Edinburgh Medical Journal, Nov.* 1863, *p.* 389.

12.—CASE ILLUSTRATING THE TREATMENT OF DELIRIUM TREMENS BY DIGITALIS.

By Dr. M. H. Sloane, Esq., Ovenden, near Halifax.

During the month of November, I was consulted by Mr. B., who appeared in a feverish and excited state. He admitted that he had been drinking for some days (I afterwards ascertained that he was doing so at the time I saw him). I ordered him a little medicine, and on my being requested to see him the following evening, I found him with all the symptoms of delirium tremens, totally unfit to attend to his business, could not sleep, and he told me he had not slept for three nights. I gave him opium with alcoholic stimulants, without the slightest effect. He talked incessantly, and fancied all manner of strange things. I was now led to try the tincture of digitalis. I gave him at night a draught containing two drachms of the tincture, and requested that he should be watched. In the morning I found him better, having slept at intervals through the night. I then gave him a six-ounce mixture, containing tincture of digitalis, two drachms, and camphor water: one ounce to be taken every three hours. He had a relapse in the evening, and I then gave him a draught containing three drachms of the tincture of digitalis. He slept that night five hours continuously, and was considerably better in the morning, but exceedingly weak, having taken no food for several days, and having suffered from sickness during the time he was taking the digitalis. I gradually reduced the dose of the tincture in his mixture, and in two days he was taking a quinine mixture and beef-tea, and was able in a few days to resume his mercantile duties.—*Lancet, Jan.* 2, 1864, *p.* 30.

13.—ON THE USE OF DIGITALIS IN THE TREATMENT OF INSANITY.

By Dr. C. L. Robertson, Medical Superintendent of the Sussex Lunatic Asylum, Hayward's Heath; Editor of the Journal of Mental Science.

[The object of giving digitalis in cases of insanity is to procure its sedative action, which is most marked in many cases. It is observed by Dr. Mayo, late President of the College of Physicians, to be the most valuable nervine sedative we possess, whilst opium is the least so. Dr. Robertson considers opium of great value in the treatment of melancholia, but as a general nervine sedative he quite endorses Dr. Mayo's observation as to the value of digitalis.]

Physiological Action of Digitalis on the Cerebro-spinal System.—The physiological action of digitalis is still undecided. The generally received opinion that it exercises a depressing influence over the action of the heart, and therefore leads to accumulation and coagulation of the blood in its cavities, has been questioned by Dr. Fuller, who, on the contrary, thus sums up his experience of the action of this medicine on the heart:—

"1. During many years, I have observed, that the cases of heart-disease most benefited by digitalis have been those in which the heart has been weak and dilated, and the pulse feeble and irregular. In these the pulse has become stronger, and steadier, and less frequent, under its action.

"2. In the only cases in which I have known death to occur suddenly during the administration of digitalis, the heart has been hypertrophied and firmly contracted. This may have been a coincidence; but, viewed in connection with the results of experiments to which I shall presently refer, it is, at least, a suspicious fact.

"3. Dr. Dickenson has pointed out (Med. Chur. Trans., vol. xxxix.), and I have repeatedly verified his observation, that digitalis, if given in full doses, induces violent uterine contraction, and checks uterine hemorrhage; and, inasmuch as its action in staying menorrhagia and uterine hemorrhage is permanent, it seems fair to conclude that it gives tone to the capillaries, and increases their contractility.

"4. This view is borne out by what I have long since observed relative to its action in arresting hæmoptysis; viz., that, whilst effecting the object required, it does not weaken, but rather increases, the force of the pulse, though it lessens its frequency.

"5. When patients die of delirium tremens, the pulse is usually rapid and fluttering before death, and the heart is found weak, flaccid, and distended with blood afterwards. These are just the cases in which, on the commonly received doctrines as to the action of digitalis, the drug ought necessarily to prove fatal, and yet modern experience has shown that in these cases it is tolerated, even in excessive doses. My impression is, that its remedial action in these cases depends on its stimulating the heart, subduing its irritability, and increasing the tonicity and contractility of the heart and capillaries, so that the brain is better supplied with blood and the effusion of its more fluid parts, which gives rise to the "wet brains" of habitual drunkards is avoided.

"6. It has been proved by experiments on animals (Dr. H. Jones) that when death is induced by digitalis, the heart is not flaccid and distended with blood, as is commonly supposed, but, on the contrary, empty, contracted to the utmost, and in a state of tonic spasm. All these facts confirm my view as to the action of digitalis; and if it is correct, its importance in relation to the treatment of cardiac dilatation can hardly be overestimated."

Dr. Arlidge, in his Report on Foreign Psychology (Journal of Mental Science, July 1863), gives the following summary of Professor Albers's researches on the therapeutical action of digitalis :—

"1. It reduces the pulse in frequency, but at the same time does not disturb the rate of breathing. Even when the heart is empty and brought to a standstill, the regularity of the respiration proceeds. 2. Alterations in the urinary secretion; an increased quantity of urine in the healthy, and still a greater augmentation when the drug is given in inflammatory dropsy accompanied by inflammatory irritation of the serous membranes of the chest and abdomen. It subdues the inflamed state of the kidneys, and restores them to their normal functional activity. In mental disturbance dependant on cerebral inflammation, especially of the serous membrane, digitalis exerts a remarkably curative effect when given after preliminary abstraction of blood and the use of antiphlogistics. 3. The solid constitutents of the urine are increased in amount, and particularly the urea, as shown both by chemical examination and by an increase of specific gravity. This alteration in the urine becomes manifest when the digitalis has reduced the frequency of the pulse and produced a feeling of lassitude. 4. Reduction of the temperature of the body, and its equalisation. 5. Vertigo, lassitude, debility, and moroseness, accompanying the reduction of the pulse by the drug. The last-named condition is a remarkable symptom associated with the operation of digitalis on the system. 6. This medicine is applicable only to those cases of madness dependent on some inflammatory lesion."

The physiological action of digitalis is a subject which would well repay a carefully conducted series of experiments. Unfortunately, I am unable to contribute to our information on this part of the question. With neither the skill, leisure, nor opportunity, to conduct such observations, like most other physicians engaged in active practice, I am content either to apply to the cure of disease, the results obtained by the scientific research of others, or, as at present, to follow the teaching of empirical experience. So far is the art of medicine yet removed from the certainty and exactness of science. The following remarks by Dr. Pereira state, I suspect, all that is yet known of the physiological action of digitalis in the presence of cerebral disease :—

"Foxglove may prove occasionally serviceable by repressing excessive vascular excitement which sometimes accompanies cerebral affections. *Furthermore, the specific influence of this remedy over the cerebro-spinal system may now and then contribute to the beneficial operation of foxglove. But the precise nature of this influence not having as yet been accurately ascertained, while the pathology of the above-mentioned diseases is involved in con-*

siderable obscurity, it follows that the therapeutic value of this influence can only be ascertained empirically."

Medicinal Use of Digitalis in the Treatment of Insanity.—I wish now briefly to state the practical results of my experience in the use of digitalis in the treatment of insanity.

Dose and Method of Administration.—With a wholesome fear of a coroner's inquest, I have not ventured on half-ounce doses, and I can report nothing as to their effect. I believe they would be too much for the average stamina of our patients. I have never given more than drachm doses; and I have usually found two or three days of such doses three or four times a day brought on the poisonous symptoms of the drug, with intermittent pulse, great reduction in frequency, and oppressive nausea. The respirations were also reduced in number; and the specific gravity of the urine lowered, and, so far as I know, the quantity increased by the use, in drachm doses, of the tincture.

[Dr. Robertson gives half a drachm of the tincture as an average dose.]

Forms of Insanity in which Digitalis has been employed.—I have, during the last year and a half, exhibited digitalis in the form of the tincture in twenty to thirty cases of maniacal excitement, recent and chronic, with varying results.

First, as to the failures. In three recent cases of mania depending on uterine excitement, two in young girls and one at the change of life, I steadily pressed the use of the drug until its poisonous effects, as shown in sickness and vomiting and intermittent pulse, were produced. The dose given was, in each case, half a drachm of the tincture three times a day. The result was simply that the patients when very sick were quiet, and that so soon as the nausea passed off the excitement returned. Again, in two severe cases of recurrent mania, I only produced sickness and depression of the pulse, and no amendment of the mental symptoms followed this physiological action of the remedy.

On the other hand, my success with this drug in cases of general paresis, in the second stage, that of mental alienation with symptoms of maniacal excitement (and in which so often in private practice aid is sought pending the patient's removal to an asylum), leads me to regard its action in controlling cerebral excitement as quite specific. I have, of course, had my share at Hayward's Heath of these troublesome cases—and how noisy and wearing they are every asylum physician knows to his cost—and they have ceased to give any trouble under the calming action of digitalis.

It is with these cases of general paresis, in the stage of mental alienation with maniacal excitement, that the assaults and injuries in asylums (which from time to time unfortunately occur) arise. There is such a reckless violence present, on which no moral or physical obstacles make the slightest impression, and this stage lasts so many weeks, if not months, that any remedy at all capable of controlling this state of things deserves a most careful trial. And such a remedy I believe we possess in digitalis, continued steadily day by day, while the tendency to excitement lasts, in half-drachm doses two or three times a day, or oftener.

It acts in every case of the kind in which I have given it as a specific, calming the excitement, and enabling the patient to pass without wear or irritation through this stage of the malady. Its action has been to steady the pulse, and thus apparently to supply the brain better with blood, and so to obviate the tendency then existing to effusion of serum, consequent on the inflammatory process going on, as we believe, in this stage of the disease in the arachnoid and pia mater. The researches of Wedl, quoted by Dr. Salomon in his able paper on general paresis, are conclusive as to the inflammatory process present in this stage of the disease.

In such circumstances the only visible result is mental quiet, and the action of the drug appears to be that of a cerebro-spinal narcotic. The functions of the stomach and bowels are not affected by its use; the appetite rather seems to improve. The pulse often remains unaffected for weeks under the use of half-drachm doses, and the only result is the specific action on the cerebral excitement. I have often found one day's intermission of the medicine bring on all previous symptoms of excitement. I have prepared a detailed history of six cases of general paresis which I have thus successfully treated. The limits of my present communication necessarily prevent my inflicting their detail on you; moreover, every member of this Association has such opportunity of testing the results of my experience, that it is sufficient for my present purpose thus generally to indicate the forms of insanity in which I advise the use of this drug.

I have also continued for many weeks with benefit to administer half-drachm doses of the tincture of digitalis in cases of chronic mania, with noisy and destructive habits. I have at this moment two such cases under treatment. In one the irritation is evidently depending on impending paralysis.

There is a third form of insanity—mania with phthisis (not unfrequent in private practice also), in which I have found the occasional use of the tincture of digitalis of great benefit. This form of mania has been so exhaustively treated by Dr. Clouston in the Journal of Mental Science for April 1863, that this reference to its treatment will for my present purpose suffice.—*British Medical Journal, Oct.* 3, 1863, *p.* 364.

14.—ON THE TREATMENT OF CERTAIN FORMS OF EPILEPSY BY BROMIDE OF POTASSIUM.

By Dr. Robert M'Donnell, Surgeon to Jervis-street Hospital, Dublin.

[The object of this paper is to draw the attention of the profession to the use of bromide of potassium as a remedy in certain cases of epilepsy. When the cases are properly selected it is a remedy of much efficacy. Sir Charles Locock, Dr. Brown-Séquard, and Dr. C. Bland Radcliffe have already recommended its use.]

Dr. Radcliffe says:—"I can testify that this remedy has proved more or less serviceable in cases the most dissimilar in character; so serviceable that the name of Sir Charles Locock ought to be remembered with gratitude by every epileptic, and by many suffering from other forms of convulsive disorders."

Sir Charles Locock, writing in 1853, says:—"About fourteen months ago I was applied to by the parents of a lady who had hysterical epilepsy for nine years, and had tried all the remedies that could be thought of by various medical men, myself among the number, without effect. This patient began to take bromide of potassium last March twelve-month, having just passed one of her menstrual periods, in which she had two attacks. She took ten grains, three times a-day, for three months; then the same dose for a fortnight previous to each menstrual period; and for the last three or four months she had taken them for only a week before menstruation. The result has been that she has not had an attack during the whole of this period. I have only tried the remedy in fourteen or fifteen cases, and it has only failed in one, and in that one the patient had fits not only at the time of menstruation but also in the intervals."

I have learned from Dr. Brown-Séquard, who has used the bromide very extensively, that he also entertains the highest opinion of its efficacy. For my own part, being full of scepticism with regard to the utility of many drugs much boasted of, and not, I must own, being rendered less sceptical by the large per centage of cures effected in the fourteen or fifteen mentioned by Sir C. Locock, I commenced using the bromide without being very sanguine as to success. I now confess that I have found it in some cases a drug of remarkable efficacy, although not so powerful in altogether stopping the attacks as it has been in the hands of Sir C. Locock."

[Several cases are then related, and the writer thus continues:]

These and other similar cases lead to the belief that we have in the bromide of potassium a remedy of considerable efficacy in epileptiform disease, when connected with uterine derangement. But in urging my professional brethren to use it in such cases, I would say that it certainly will not be found successful in every case, even of epilepsy connected distinctly with menstrual derangement. I have administered it, with the consent of my colleague, Dr. Banon, to a young woman in the Montjoy Female Convict Prison, who had puerperal convulsions at the birth of her first child, and has since been epileptic, her attacks occurring for the most part at the menstrual period. In this case although I expected much from it, I cannot say that any substantial benefit has arisen from its use, yet it has been given perseveringly and in large doses.

With reference to the dose, from a not inconsiderable experience in the use of this medicine, I can state that it may be given with perfect confidence and safety, in much larger quantities than it is usually prescribed. I have given thirty or forty grains, and even more, three times a-day, for months without observing any bad results; and of this I am certain, that often such a dose as ten grains, three times a-day, is too small to develope any good result.

Although the independent testimony of several practitioners points to cases of epilepsy, with derangement of the uterine functions, as those in which the bromide of potassium is most likely to be beneficial, yet there are others in which its effects are unquestionably good.

A lad, aged eighteen years, was admitted to the hospital of the Montjoy Convict Prison, on April 12th, 1863. He was said to be epileptic, having three or four fits in a day. I myself never saw him in one; but at the time of my visit I found him dull, stupid, and slow of speech. I was struck by the peculiar odour exhaled from the skin of this patient; it was the same mixture of garlic and brass noticed in a former case; and it was the similarity in this respect alone that suggested to me the idea of giving him a medicine which had been so useful in the case alluded to. I commenced with ten-grain doses of the bromide three times a day, increased after one week to fifteen, and in a fortnight to twenty grains. This boy apparently completely recovered, so much so that he was not recommended for removal to an associated prison, as is usual with confirmed epileptics; and indeed not having myself seen him in any attack, I was inclined to doubt the correctness of the diagnosis, and regard him as a malingerer. My friend, Dr. Mayne, however, who had one day accompanied me to visit a patient in hospital recognized the lad as having been formerly a patient under his care in the South Dublin Union Workhouse. On his assurance I gave up the notion of the boy being a malingerer, and I now think his case may fairly be regarded as benefited by the treatment.

I do not mean to say that the peculiar odour observed is a sufficient indication whereby one could venture to predict anything like successful treatment, yet the circumstance is worthy of observation; a peculiarly fetid odour from the skin and discharges is common in epileptic patients, as a forerunner or accompaniment of a series of attacks. One can foretell a coming series by the odour of the discharge from an issue or a burn; but the smell to which I have alluded seems to me something quite *sui generis*, and as I have already said is best compared to that of sublimed arsenic.

When epileptiform attacks are traceable to sexual excess in males, I have reason to state with some confidence that the bromide will be found useful; but as my observations on this subject are as yet incomplete, I must look forward to a further communication concerning it on a future occasion.—*Dublin Quarterly Journal, Feb.* 1864, *p.* 43.

15.—A CASE OF TRAUMATIC TETANUS TREATED BY NICOTINE.

By Dr. JOHN W. OGLE, Assistant-Physician to St. George's Hospital.

[The following case was treated by Mr. Holmes and Dr. Ogle conjointly.]

The patient, Mary G., a thin, delicate-looking girl, aged fourteen years, was admitted into St. George's Hospital on August 29, 1863, having a long lacerated wound of the soft parts of both sides of the left leg, which had been caught in the spokes of the wheel of a cart. The edges of the wound were much bruised, and a few days after her admission began to slough. Subsequently, the wound became cleaner, and her general health improved under the use of tonics and occasional sedatives, until the evening of September 6, when she complained of what was called "slight sore throat." Croton oil and colocynth were given. On the following morning (September 7) there was trismus, the mouth was firmly closed, and the risus sardonicus existed. The lips were dry, the pulse small, weak, and 156 per minute. The pills had acted once. At noon

I was desired by Mr. Holmes to see her with him, and found her lying flat in bed, with the head raised, the eyes half closed, the lips tightly drawn across the gums, the upper teeth and gums being freely exposed. She was utterly unable to open the mouth, and the muscles of the face were perfectly rigid, especially the masseter and buccinator on either side, and the muscles of the shoulders and back of the neck. The skin was cool; the pulse feeble, and about 135 per minute. The intellect was quite clear. Both the pupils were rather large, but equal, and acting to light. Ordered a repetition of croton oil and the compound colocynth pill, and a lotion to the wound, consisting of half an ounce of extract of belladonna to a pint of water. At 5 p.m. the bowels had acted very freely. At this stage I talked with Mr. Holmes on the advisability of amputating the limb, seeing that, owing to the nature and extent of the local injury, it would have been quite impossible to have divided separately such nervous trunks as supplied the parts implicated. Mr. Holmes consulted with two of his surgical colleagues, Mr. Pollock and Mr. Lee, and, in consequence, the idea of amputation was rejected, as Mr. Holmes said, on the following grounds:—"1. We thought that it was too late, as the irritation, or the diseased condition, or whatever you like to call it, would be propagated by this time up the nerves, far beyond the point of section in amputation. 2. We did not think it impossible that the patient might survive without amputation. 3. She seemed so weak, that amputation would be a serious danger to her, as would (I think) the necessary chloroformisation. 4. No success has, as yet, attended amputation of the thigh, I believe.

September 8, 9 a.m. There had been very little sleep, but the patient had swallowed between one and two pints of beef-tea. There had been no spasmodic action of the muscles of the back or belly (opis- or emprosthotonos). In all important points the symptoms were the same as on the evening before. Pounded ice was ordered to be applied along the spine enclosed in a piece of intestine. 11.30 a.m. The risus sardonicus had increased, the cheeks were flushed, but the rest of the face was pale. At her own request she was raised in bed, but the attempt to swallow a teaspoonful of porter, which she wished for, brought on spasm of the muscles of the neck and face, and she swallowed it with great difficulty; a few minutes afterwards she swallowed it with great ease. The respiration was quiet, at 36 per minute; the pulse 140. The teeth could be separated a little. As much strong beef-tea and wine as possible were ordered to be given; and it was agreed to try half-drachm doses of spirits of turpentine with the acacia mixture and water, every four hours. One dose only could be given, owing to the amount of distress which it caused. At 3 p.m. it was agreed by Mr. Holmes and myself to try nicotine; and, accordingly, after procuring a genuine specimen from Messrs. Bullock and Reynolds, we proceeded, at 9.15 p.m., to give four drops of a solution containing half a grain, in ten minims of water, in a little brandy-and-water. At this time the forehead was much wrinkled; the abdominal muscles, as well as those of the neck and shoulders, were very tense. There was no hiccup, but great difficulty in swallowing. The intelligence was unimpaired; the pupils dilated as before, but acting readily to light. Pulse 140, and the skin very cold. The urine was passed along with the fæces. At 11 p.m. she stated that she felt altogether easier, and the general aspect was certainly improved. The pulse was 144, and the skin perfectly warm. The heart's action was greater than it had been. There were no spasmodic seizures, but the muscles of the abdomen were still very rigid. There was no increased perspiration. It was determined to administer an enema containing eight drops of the above-mentioned solution of nicotine, along with beef-tea and a little brandy; and it was ordered that a similar injection should be exhibited at two o'clock in the course of the night. The first injection was retained for an hour without producing any particular result, as was also an injection containing egg and brandy and beef-tea. At 5.30 a.m. on the following morning the nicotine enema was repeated, but rejected almost entire.

9th, 9 a.m. There had been no sleep, and slight "wandering" had existed in the night. The patient complained greatly of the throat, and the tongue was very sore. The neck was very rigid, and the muscles of the lips stretched as before. Occasionally, general twitchings of the entire body existed, and especially starting of the injured leg. The patient raised the arms readily, and held on by the bed-rope as usual. No perspiration had existed, but the skin was warm; pulse 124. I gave the patient eight drops of the nicotine solution with brandy and water, and desired it to be repeated at twelve a.m. The first of these doses was retained in the stomach; the second could not be swallowed, owing to spasm of the throat, and, in its place, I ordered twelve drops of the solution to be administered with beef-tea and egg as an enema. About this time (12.30 a.m.) attacks of opisthotonos came on (from four to eight in the hour), during which she had the sensation of falling down. These attacks were not very severe, though sufficiently marked, but became considerably intensified when attempts were made to give her the medicine. Much spasm also about the throat and "frothing" at the mouth was occasioned. Very little of the medicine was swallowed, but she could open the mouth about a quarter of an inch. A nutritive injection was retained. The pulse was regular, and 136 per minute. At 12.55 p.m., twelve drops of the nicotine solution was administered in a small enema. No spasm arose during its exhibition.

Subsequently, Mr. Holmes and myself determined to try the use of the nicotine by subcutaneous injection; and accordingly, at 2.20 p.m., five minims of it, along with five of water, were injected under the skin of the left arm, which caused a slight blush of redness over the part where the injection was made. At 2.30 an attempt to swallow some porter created excessive spasm. At 3.15 the redness occasioned by the injection had nearly disappeared. At 3.30, attempts to swallow produced violent spasm of the muscles of the throat, lasting about five minutes. At 4 p.m., the arrival of the patient's mother caused much agitation and general spasm. At 4.30, Mr. Holmes injected ten minims (equal to half a grain of nicotine) into the tissues at the back of the neck. At 5.40, the same injection (ten drops) was repeated in the left arm, and was followed by some spasm. Subsequently an attempt was made to pass a flexible gum catheter through one of the nostrils, and down into the œsophagus, so as to give a nutritive injection, but most violent and long-continued spasms, with great lividity of face, and apparent suffocation, were in consequence, produced, and the fluid was returned through the mouth. At 5.55, the abdomen was less hard and tense. At 6.30, pain was complained of at the præcordial region, and the administration of a beef-tea injection induced a most severe general spasm, the injection being at once returned. At 7.15, a quantity of the nicotine solution, equal to half a grain, was again injected under the skin. At 8.15, it was reported that since the last injection there had only been slight twitchings of the facial muscles, and the teeth could be separated a little more. Pulse 140 per minute. A few drops of water were swallowed with less spasm. At 8.40 the subcutaneous injection was repeated. At 10 p.m. the patient was reported to have been more restless. The pulse was 154 per minute. The skin somewhat perspiring. 11 p.m., the restlessness continued. The abdomen was much softer, the muscles being less tense and rigid, but she seemed weaker. Pain at the throat complained of. 11.15. Twenty drops of the nicotine solution (equal to one grain) were injected into the subcutaneous tissues of the right arm. During the operation spasm came on, and gradually increased in intensity, and so affected the respiratory muscles that the face became livid, and breathing was altogether arrested; but after cold water had been dashed on the face, and vapour of ammonia applied to the nostrils, she gradually came round, the pulse being 164 per minute. 11.30. The breathing was long drawn, and of a sighing character. Severe general spasm returned, lasting about four minutes. By slight pressure upon the lower jaw the mouth could be opened to the extent of half an inch. At 11.44, the breathing was sighing, and irregular as to rhythm. Pulse 144, regular.

10th, 1 a.m. Breathing the same. Pulse 150 per minute. There was a little moisture on the palms of the hands. The abdominal muscles were quite lax, but the general spasms were so continuous, though not violent, that it was thought advisable to repeat the injections of nicotine. The pupils were almost quite inactive under the influence of light. The breathing continued to have an irregular character until about 3.30 a.m., when it became very much impeded, and the spasms continued throughout, though slight as to intensity. She died at 4.40 a.m.

Post-mortem examination was not permitted.

Such are the painful records of this case, in which the use of nicotine proved unavailing. Looking to the history and details of the cases described by Dr. Haughton, I was at first sanguine as to our results in this recent one, especially as at one time some extenuation of the severity of the symptoms appeared to show itself. Possibly the local injury, considering the tender age and delicate constitution of the patient, may have been so extensive that its results would have been intractable under any mode of treatment. Possibly, again, the nicotine may have been not so potent as I had thought, or I may have been too sparing in its administration, being fearful, as I naturally was, of giving it in over-large quantities, knowing what evils have, in other hands, attended the use of tobacco when carelessly or improperly used.

Of course, all these supposable causes of failure in the use of nicotine may equally be assumed in the case of other remedies which are so frequently resorted to, and with a like want of success; and are sufficiently probable to allow of hope (considering the partial success which has appeared to attend its use as related by Dr. Haughton and others, and also the results of the use of tobacco in tetanus), that the nicotine may yet prove a serviceable weapon in our hands against a disease, in connexion with which more than with any other, except, perhaps, hydrophobia, is the aphorism—"Vita brevis, ars longa, occasio præceps, experientia fallax, judicium difficile"—more justly applicable.

As regards the particulars—the individual symptoms which occurred in this case I will make no further observation, as such would be foreign to the scope of these remarks.

Before closing, I cannot refrain from quoting a few lines from Mr. Simon's note before mentioned, in which he further touches on the treatment of tetanus. After referring, as above stated, to the successful treatment of tetanus by the tincture of the sesquichloride of iron, he proceeds to remark,—"Once I treated a case of acute tetanus with belladonna. I think the drug was amply tried, but I am not sure that the case (which, *perhaps*, was one of centric tetanus) was quite the fairest which could have been selected, *i.e.*, on the theory which would seem likeliest to justify the trial of belladonna." Again, Mr. Simon, alluding to the general treatment of tetanus, says:—"The other day, at Constantinople, Marko Pasha, the Sultan's doctor, told me that he had cured a large number of cases of tetanus—I believe nine out of eleven—by the use of opium, which he rubs in right and left, and gives in enemata, till profound sleep (? coma) results from it. His quantities were large; but, he said, none had died of opium, and all who slept recovered."

These remarks, though not directly relative to the immediate subject and purpose of this communication, are so interesting, that I could not desist from quoting them in this place.—*Med. Times and Gazette, March* 12, 1864, *p.* 278.

16.—TETANUS FROM BURN TREATED BY THE APPLICATION OF TOBACCO TO THE RAW SURFACE OF THE WOUND.

By — Tyrell, Esq., Surgeon to Jervis-street Hospital, Dublin.

[The following case was read by Mr. Tyrell before the Surgical Society of Ireland. The patient was six years of age, and had a burn extending from the elbow to the wrist. Cotton-wadding was at first applied to the saw surface, and an anodyne was administered. There was no pain in the arm unless moved. In rather less than three weeks well-marked symptoms of tetanus set in.]

As his bowels were confined, I directed five grains of calomel to be given at once, and in three hours ten grains of compound powder of jalap. I also took off the dressing from the arm, which was partly healed, but in three separate spots small ulcers were present. I then made an infusion of Cavendish tobacco by infusing half an ounce of cut Cavendish in half a pint of boiling water, and when the infusion became sufficiently cold, I rolled the arm up in lint saturated with the infusion, and directed the lint to be kept constantly wet. Wine and strong beef-tea to be given during the night in small quantities and often.

On the morning of the 9th the night nurse stated the bowels were well freed, and that the child had slept for an hour, and had occasional spasms during the night. He took four ounces of port wine and the same quantity of beef-tea. He looked, however, worse, the tetanic expression better marked, and the muscles of the neck and abdomen if possible more rigid than on yesterday evening. I ordered the lint on the arm not to be changed, but to be constantly moistened with the tobacco infusion, and the wine and beef-tea to be continued. On the 10th the resident, who saw him frequently during the night, reported that he slept for three hours, and took without much pain six ounces of wine and half a pint of beef-tea within the twenty-four hours. The tetanic face is less marked, he opens his mouth wider, and on the whole there was a marked change for the better; I directed the same treatment. On the 11th improvement more marked; took six ounces of wine and one pint of beef-tea.

12th: Much better in every respect; took the same quantity of wine and beef-tea.

On the 13th I considered him out of danger, as he could open his mouth almost to the full extent, was quite gay, and asked for a chop, which I readily granted. As there still remained some rigidity of the neck and abdomen, I continued the application of the tobacco until the 15th, when I removed the lint, and carefully washed the arm, which was quite healed, except in one spot about the size of a sixpence. There still remained the rigidity of the arm, nor was the expression of the face natural, and indeed, strange to say, the arm is yet rigid, but not at all painful.

In bringing this case before the Society, I do not wish the members to think I look upon it as one of acute tetanus, but it was a well-marked case of subacute tetanus, as my colleagues can testify.

It is now more than forty-two years since Mr. O'Beirne, surgeon to the hospital to which I have the honour to belong, called the attention of the profession to the use of tobacco, and since that time it has been occasionally used. But it was not until the researches of the Rev. Dr. Haughton in 1856, upon the physiological action of nicotine and strychnine upon frogs, drew attention to the power which the former substance had over poisoning by the latter. In March, 1862, he read his well-known paper before the College of Physicians, upon the treatment of tetanus by nicotine, and since that time tobacco is no longer used empirically, but on well-known physiological principles.

It occurred to me when the subject of the present case became affected, that if tobacco or its active principle was of service when given internally, *a priori*, it should be more so in peripheral or eccentric tetanus, if applied locally, by paralysing the nerves of the affected part from which the irritation proceeded, and thus remove the cause of the tetanic spasms. That it did so I believe to be the case at any rate. The attempt was based upon scientific principles, and the patient got well—spontaneously, some will say. Future investigations can alone determine the question. In considering if the same plan might not be adopted in cases of acute traumatic tetanus arising from a small wound, where a decided impression should be made quickly, it strikes me that a solution of nicotine of a known strength applied to the wound might be useful, and perhaps in the idiopathic form, if the cuticle on the back was raised by a blister, the endermic use of the remedy would more quickly bring the cord under its influence, and perhaps with less vital depression than when given internally.

Mr. Poland, in the article on tetanus in Holmes' "System of Surgery," states that the muscles of the jaw first attract attention, and likewise cites the authority of Mr. Morgan. As far as I have seen, I believe the muscles of expression are affected before those of mastication (I believe my colleague, Mr. S., who has seen a great number of cases, is of the same opinion), and they are the last to relax.

The mode in which the tetanic symptoms came on is unusual: the spasms affecting the limb injured first, and preceding by several days the symptoms elsewhere. This is rare. Mr. Morgan, in his work on tetanus, relates two such cases as having occurred in Guy's Hospital.

Knowing how successful stimulants were in the case read before this Society at a former meeting by Mr. Stapleton, and in how many cases death takes place from exhaustion, I gave a liberal allowance of wine from the beginning, as I am confident, no matter what medical treatment we use, the strength of the patient should be husbanded.

Dr. KENNEDY wished to know the extent of the surface exposed to the tobacco infusion.

Mr. TYRRELL said the arm on the inner side, from the elbow to the wrist, was subjected to the infusion, and that the surface was raw at the time he applied the poultice to it.

Dr. KENNEDY said he asked the question, because some time ago he saw a child very nearly poisoned by the application of belladonna. That the tobacco infusion did good in this case was clear enough, but so powerful an agent should be used with great caution. He did not hear the strength of the infusion.

Mr. TYRRELL—It was half an ounce to half a pint; but he forgot to say that he did not leave the child under the influence of tobacco without taking proper precautions; for the resident had directions to attend to the pulse, and if the child showed any symptoms of poisoning by tobacco, either by vomiting or depression of the heart's action, to discontinue it. The reason he applied the tobacco locally was, that a child of such tender years could not bear up against the powerfully depressing action of nicotine, and knowing that morphia and other agents had been applied to the raw surface with effect in cases of neuralgia, he thought there was no better way of giving it than by acting on the part directly involved. They all knew from the records of that Society that in most of the cases of tetanus which had been cured, the liberal administration of stimulants had been adopted from the beginning, and where the patient did not get stimulants or was unable to afford them, the case turned out badly.—*Dublin Med. Press, March* 2, 1864, *p.* 213.

17.—CASE OF EMPROSTHOTONOS (?) CURED BY THE USE OF ICE TO THE SPINE.

By Dr. JAMES EDMUNDS, London.

The subject of this report is a married woman, 28 years of age, healthy and intelligent, altogether free from flightiness or hysteria, and of good family history. Her second child is nine months old, and at the breast.

On January 21, she retired to bed quite well, but on rising next morning at 7.30 her fingers were slightly benumbed, and between nine and ten o'clock while dressing the baby, her fingers suddenly "turned in," so that she was obliged to huddle the child up in her arms to save it from falling. This "turning in" of the hands increased rapidly, and caused a cramp-like pain in the arms; and her lower limbs from the knees downwards also became affected in a similar way, but less severely. In about half an hour the pain was so great that she was obliged to sway herself about to try to get ease, and she was so helpless that her husband first lifted her into an easy chair and then fetched a bed down stairs, and laid her upon it. The pain increased, and she could not help moaning and screaming, although her mind and her manner continued clear and rational.

Between 11 and 12 o'clock she was seen by Dr. S. C. Griffith, of London-wall. There was then extreme and rigid flexion of the hands and arms, and her head was so drawn forwards that the backs of the wrists were fixed close to her mouth; the tongue was coated; the pulse was not remarked. Some aperient medicine was ordered, of which she took one dose, and after which she was slightly sick.

At 1 p.m., Dr. Webber saw her in consultation with Dr. Griffith, and Dr. Webber noted her pulse as "slow and weak —about sixty." Sulphuric ether and anti-spasmodics were ordered at once, and a dose of morphia after a few hours in case the pain did not abate. She had intervals of comparative ease, but, nevertheless, continued to grow worse; and at 5 p.m., seven minims of "sedative solution of opium" were administered. After this, all her symptoms were aggravated; and during the evening she suffered so much that her screams could be heard across the road. Her neck became turgid, her respiration somewhat constrained, and she was becoming rapidly exhausted.

At 10 p.m., Dr. Griffith asked me to meet him in consultation; and I did so. At this time her condition was certainly most remarkable. I had seen plenty of screaming hysteria, and a few cases of opisthotonos; but I had never seen a case precisely like this one. She lay upon the bed on the floor on her left side, with her wrists in contact with her mouth, as has been described; her spine was bent forward, and her knees bent and drawn upward, but not to the extent of coming into actual contact with the abdomen. There was an expression of great anxiety and pain upon the face, and the lips were sufficiently retracted to show the teeth; but the expression was not exactly the risus sardonicus which I have seen in opisthotonos, and she could swallow by making a great effort to ignore the tearing, cramp-like pain which occupied her, and which forced from her a continual moaning scream. Her cry was nothing like the passionate scream of hysteria. She was quite rational, and she merely swayed herself about instinctively. Her forearms were hard and rigid, and the wrists were so fully flexed that that her hands were forced open, and her legs were similarly affected, but in a much less degree. It was stated that a considerable quantity of pale urine had passed from her. The pulse could not be judged on account of the spasm of the neighbouring tendons. We had the patient held up on to her feet. She could not stand, and her legs were about as much cramped as in a bad case of cholera; but they were not in a state of rigid spasm like that of the forearms.

There was no appearance of hyperlactation; no evidence of albuminuria or uremic poisoning; no history of worms; no traumatic injury, and no clue whatever to any source of eccentric irritation excepting the single fact that her bowels had not acted for three days.

I had never seen a case of emprosthotonos, but this one could be referred to no other term. Certainly it was not a case of hysteria, and, moreover, it was manifest that, unless speedily relieved, she would die either from exhaustion or spasm of the respiratory muscles. There was evidently some exaltation of the reflex irritability of the spinal cord or some acute cause of nervous irritation, and the indications for treatment were, first, to clear out the bowels; second, to lessen the irritability of the cord, so as to do away with this frightful state of spasm.

The first object was effected by at once placing upon the tongue a purge, consisting of one drop of croton oil and five grains of calomel. An enema of turpentine and castor oil was also administered to free the lower bowel from flatus or fæculent accumulation, and the purge was to be repeated in three hours. How was the second object to be accomplished? The inhalation of chloroform, on the one hand, or the application of ice to the spine, on the other, appeared to be the only remedies adequate to the emergency, and as there were some objections to using chloroform, we determined to use the ice. This was accomplished effectually by her husband, who had two ox gullets alternately filled with ice, and kept it continuously applied to the whole length of the spine for seven hours, when all her severer symptoms had disappeared.

The screaming and moans were at once mitigated by the ice, and in about half an hour ceased entirely. In two hours her feet became flexible. About 8 a.m. on the 23rd, the spasms had so far subsided that she could open her hands, but the numbness remained. She felt very sore in her muscles when turning in bed, especially in the forearms and legs, and in the back. Her tongue was cleaner, and the pulse had increased in volume, and had risen to 76. On the morning of the 24th the numbness entirely disappeared, and afterwards she had no return of cramp or spasms, but a little subacute bronchitis and hoarseness hung about her for ten or twelve days longer. She is now in her usual health.

In this case the patient herself ascribes the relief of her dreadful pain and cramps entirely to the use of the ice; and although it is difficult to distinguish between the mere *post hoc* and the *propter hoc*, yet I cannot but believe, if ever I saw a life saved by therapeutical application, that this patient's life was saved by the use of the ice in this way to the spine; and the case strongly supports the theory propounded by Dr. Chapman, of Somerset-street.

If it be really a fact that, by the application of cold or heat to particular portions of the back, we can directly exalt or lessen the reflex activity of the grey matter of any given segment of the cord, or of the sympathetic ganglia which govern the blood-supply of distant parts, we shall have made a rare and philosophical hit in therapeutics, and, in the discriminating application of cold or heat to the nervous system, possess a remedy which may prove useful in almost all diseases. I had seen Dr. Chapman's brochure on the subject of this discovery, and also his paper in the Medical Times and Gazette, but thought the idea too pretty to be anything more than a plausible theory, until my own child being in great danger from an obstinate laryngismus, connected with dentition, I tried "the ice bag" to the cervico-dorsal portion of the spine, at the suggestion of Dr.

Ramskill, and it has certainly done more to keep off the strangling attacks than anything else.

Dr. Ramskill has not only adopted this treatment largely, but speaks of it to me with the greatest confidence in the treatment even of epilepsy, and in his practice it seems to be the means of doing great good, and in many cases of keeping the fits almost entirely in abeyance.

For the application of the ice to chronic cases there is a great practical difficulty in respect to obtaining perfectly waterproof bags of the proper shape. Dr. Ramskill was kind enough to give me one for my own child, and since then I have communicated with Dr. Chapman himself, who most politely forwarded me, at prime cost, some bags prepared under his direction, and these answer very well.

I am indebted to Dr. Griffith for much of this report.—*Med. Times and Gazette, March* 12, 1864, *p.* 280.

18.—ON SOME CASES OF LOCAL PARALYSIS.

By James Paget, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

[In the first part of this paper, which is the report of a clinical lecture delivered at St. Bartholomew's Hospital, Mr. Paget relates some interesting cases of local paralysis. All these cases resulted from injury to the nerves supplying the paralysed part, not from injury to the nerve centres. Several are paralysis of one arm from injury to the brachial plexus, or the large cords in the arm, and these are most commonly met with. Similar injuries to the leg are less frequent.]

In these cases the loss of power and the defect and disorder of sensation were so much greater than any other consequences of the injury, that one cannot doubt a predominant injury of the nerves in the forearm. But its effects were complicated by the damage of the muscles and fibrous textures, and the consequent hardening and adhesion of their structures.

From all the cases together, it is evident that long-continued paralysis of both motion and sensation, attended with rapid wasting, and more or less of neuralgia, may ensue as a consequence of such injury of nerve-fibres as, probably, falls far short of rupture or destruction of their substance, and that these things, in an extreme degree, be due to an injury which, neither in its manner of occurrence nor in any other attendant character, would seem to have fallen with special severity on the nerve-trunks. It would seem as if the nerve-trunks might be rendered long incapable of their functions by such injury as, when occurring to the brain or spinal cord, we call concussion.

I am not acquainted with any case of the kind in which actual examination showed the state of the nerves. The exact pathology of the case is therefore unknown to me.

But of their treatment. It must be somewhat different in the cases of simple paralysis and in those that are complicated with the inflammatory hardening of textures, neuralgia, and other various symptoms.

For the former—for that of the man in the Hospital, for example,—the main design must be to maintain the nutrition of the limb, notwithstanding its inaction. And for this purpose there must be provided, in addition to whatever may be required for the maintenance of vigorous general health,—

1. Constant warmth of the limb; the whole body must be warmly clothed; the paralysed limb and its fellow especially so. No good or quick repair will take place in a cold limb.
2. Regular friction and shampooing, especially circular champooing.
3. Localised galvanism, that every several paralysed muscle may be made to contract.
4. Constant voluntary efforts; constant endeavours to attain every lost movement; and when any such endeavour is effectual, frequent exercise of the recovered power.
5. Swedish gymnastics, *i.e.*, set exercises for each muscle in which power is not wholly lost.
6. Guards against distortion from unbalanced actions of muscles, especially contracted fingers or elbows.

With these things patiently, that is, year after year continued, much good may be achieved. In the case already mentioned as exactly like that of the young man in the Hospital, great good has been gained by a year's treatment; she can now move the elbow in flexion and extension, and draw the upper arm inwards, and slightly bend and extend the fingers. There is very distinct sensation in the whole arm and hand, and freedom from all pain; the natural temperature has been restored, and she has ordinary sensations of heat and cold. Galvanism seems to have done special good; the arm was observed to become weaker when it was omitted for a week.

But for the more complicated state, though the treatment must be essentially the same, it must be gentle, with less of galvanism, and less force of movement, and less of voluntary exercise, for these can rarely be borne as they can be in the cases of simple paralysis.

In both the cases which have been referred to of complicated injury to the structures above the wrist, much improvement has taken place. Two years after the accident, the first of these cases was reported to me as being much better; and of the second, the last note that I have (more than two years after the injury) is, that the hand was much better; the thumb and forefinger could be easily brought together, and all the fingers had become much more moveable.

Two symptoms referred to in some of the preceding cases deserve particular notice; namely, what I have called the glossy state of the fingers, and the inequality of the pupils.

Glossy fingers appear to be a sign of peculiarly impaired nutrition and circulation due to injury of nerves. They are not observed in all cases of injured nerves, and I cannot tell what are the peculiar conditions of the cases in which they are found; but they are a very notable sign and are always associated, I think, with distressing and hardly manageable pain and disability. In well-marked cases, the fingers which are affected (for this appearance may be confined to one or two of them) are usually tapering, smooth, hairless, almost void of wrinkles, glossy, pink or ruddy, or blotched as if with permanent chilblains. They are commonly also very painful, especially on motion, and pain often extends from them up the arm. In most of the cases this condition of the fingers is attended with very distinct neuralgia both in them and in the whole arm, and its relation to disturbance of the nervous condition of the part is, moreover, indicated by its occasional occurrence in cases where neuralgia continues after an attack of shingles affecting the arm. In two such cases I have seen this same condition of the fingers well marked, and only very slowly subsiding, and seeming unaffected by the ordinary treatment of neuralgia.

The inequality of the pupils is well marked in the man now in Darker Ward. The pupil on the injured side is always smaller than the other, but they both act equally well, and there is no defective sight. The same condition has existed ever since the injury in the young lady whose case I related next after his. Her right pupil, corresponding with the paralysed right arm, is always smaller than the left, and there is a very slight appearance, which is said to be increased when she is not in perfect health, as if the right eye were a very little smaller or less open than the left.

My attention was called to the existence of this inequality of the pupils by Dr. Hughlings Jackson, who suggested also what seems not improbable, that it may be due to the relations of the brachial plexus to the cilio-spinal portion of the spinal cord.—*Medical Times and Gazette, March* 26, 1864, *p.* 332.

19.—ON THE CURE OF NEURALGIA BY ARSENICAL PREPARATIONS.

Dr. Cahen, Physician to the Jewish Hospital founded in Paris by M. de Rothschild, has given arsenic with success to sixty-five people suffering from neuralgia :—

Facial neuralgia	35
Sciatic ,,	8
Intercostal ,,	4
Otic ,,	2
Dental ,,	2
Epigastric ,,	14
	65

In the two cases of dental neuralgia, the patients had had several teeth extracted in vain. One young lady amongst others had had eight teeth pulled out. M. Cahen gave her arsenic, and the cure was both successful and rapid.

From January 1859 to 1862, M. Cahen has given arsenious acid for different causes to 292 patients, and the dose of acid was per diem at a minimum of 0·015 grains, and at a maximum of 0·6 grains. The minimum of the dose for treatment was 0·27 grains, and the maximum 6·075 grains. The arsenious acid was given sometimes in pills, sometimes in lotions, with a solution of one in a thousand, sometimes in baths, with 15·43 grains of arsenite of soda, for gouty and rheumatic affections.

M. Cahen has observed about neuralgia that sciatic neuralgia was that in which the effect of arsenic was less evident. However, this assertion should not prevent practitioners from using this medicine for sciatica, and they will find as an encouragement a good example of a cure which Dr. Barella communicated to the Society of Natural and Medical Sciences at Brussels.

Of the 292 patients treated at the Rothschild Hospital, M. Cahen noticed diarrhœa in 19 cases, but not so dangerous as to stop the arsenical treatment.—*Journal Méd. de Bruxelles.—Dublin Medical Press, March* 2, 1864, *p.* 222.

20.—THE EXTERNAL EMPLOYMENT OF ACONITINE IN NEURALGIC PAINS.

By Dr. A. B. Garrod, F.R.S., Physician to King's College Hospital.

Aconite is a most valuable external remedy; and for the purpose of alleviating neuralgic pains, I know of nothing equal to it. In the form of the Unguentum Aconitiæ, in which eight grains of the alkaloid are dissolved in a little spirit, and then thoroughly mixed with an ounce of prepared lard, it is a clean and elegant preparation.—*Medical Times and Gazette, Feb.* 6, 1864, *p.* 146.

[In our own practice, we have lately found some of the concentrated tinctures, prepared by some of the American chemists, very valuable additions to the medicines usually employed by British practitioners. In neuralgic affections, we have found the most remarkable efficacy from the concentrated tincture of gelseminum (the yellow jessamine), in doses of from five to ten drops. It seems to possess properties somewhat similar to aconite, but without the depressing effects of that drug. We would strongly recommend our fellow practitioners to try this medicine in their neuralgic cases.—EDS.]

DISEASES OF THE ORGANS OF CIRCULATION.

21.—ON DIGITALIS IN CARDIAC DISEASE.

(From the "Hospital Notes" of the Medical Times and Gazette).

[In our last volume, several articles were given showing that Digitalis is a cardiac tonic.]

Dr. SUTTON, in order to test the value of this conclusion, has prescribed tincture of digitalis in several forms of heart disease occurring amongst the out-patients of the Metropolitan Free Hospital. The following are the results of his observations:—

1. In cases of mitral regurgitation, with dilated hypertrophy and enfeebled condition of the heart, digitalis gives great relief, as in the following case:—A woman, aged thirty-eight, was so weak that she was led into the out-patient's room. Her feet and ankles were œdematous. Her pulse was very feeble and quick, but regular, and the heart's visible impulse was diffused and indistinct, and the apex was felt over the seventh rib. The first sound was inaudible at the base, and the second was of a very high pitch. There was a faint systolic bruit at the apex. Her complaint was, in her own words, "I feel so very faint." Fifteen drops of the tincture of digitalis, with the like quantity of tincture of the sesquichloride of iron, was given in an ounce of water three times a-day. Four days later she walked into the room without help, and said she felt much better. The bruit was more audible. She continued to do well.

2. In a case of chronic pericarditis, with very great effusion into the bag of the pericardium. A man aged thirty, complained of shortness of breath and great faintness. He was unable to move in bed without feeling very faint; his lower extremities were œdematous; his pulse was very feeble and quick. The

only sound of the heart that could be heard was the very faint, distant-like click of the second sound at the base. ℞. Tinct. digitalis, ♏xxv.; eth. chlor., ♏xv.; tinct. ferri mur., ♏x.; ter die s. Five days afterwards he expressed himself relieved. Pulse regular and stronger; no perceptible difference in the extent of dulness on percussion. The urine was not increased. The digitalis was continued for six weeks. He recovered so far that he could walk about the house, but he had a relapse, and died four months later. A post-mortem confirmed the diagnosis.

3. In aortic regurgitant disease it requires to be given with great caution. In one case, while taking digitalis, the patent's pulse became irregular, increased in frequency, and he died, with well-marked symptoms of intra-cardiac blood concretion.

4. In heart-disease, with dropsy, it exercises no well-marked influence over the secretion of the urine.

5. In one case of aortic obstruction, with simple hypertrophy, and a strong, heaving impulse, digitalis was given in fifteen-drop doses three times a-day for fourteen days, when the patient remarked, "I feel the action of my heart much more troublesome when I take the mixture."

6. That in a case of aortic regurgitant disease, complicated with angina pectoris, the patient having an atheromatous physiognomy, digitalis was given for fourteen days. No relief to the pain; pulse irregular.

7. Digitalis is very beneficial in cases of the following class:—Men of middle age, of the sanguine temperament, and, of the neuro-vascular diathesis, as described by Professor Laycock; complaining of dyspnœa on exertion; very weak; appetite normal; pulse rather quick, easily excited, and soft; heart's impulse seen in the epigastrium, feeble to the hand; first sound inaudible at the base; second sound, high pitch; at the apex, the first sound faintly heard. In one such case there was albuminuria; in two arcus senilis. All these cases improved quickly while taking tincture of digitalis and iron. That it was not the effects of the iron was proved by the fact that they all had improved after taking the mixture three or four days.

8. It may be given cautiously for six or eight weeks, in fifteen-drop doses in cases of enfeebled heart with mitral disease, without producing any irregularity of the pulse, but the reverse in aortic disease.

It will be seen from the above conclusions, that Dr. Sutton confirms Dr. Handfield Jones' remarks, that the most suitable cases for the administration of digitalis are those of enfeebled heart; that digitalis "seems to be injurious in sthenic, and beneficial in asthenic cases." The same with respect to Dr. Corrigan's statement (to which Dr. Leared called the attention of the Profession), that in cases of aortic disease the effects of digitalis are injurious.

We will now give the experience of another Hospital Physician, Dr. WILKS. Going round with him a few days ago, we saw at least a dozen cases of heart disease which had their origin in a former attack of rheumatism. When there was dropsy, the ordinary treatment was adopted; and when the heart was weak and irritable, iron and digitalis appeared to be Dr. Wilks' favourite combination. He said that, as no medicine could alter the changes which had occurred in the valves, the only remedies which can be of use are those which tend to preserve the power of the muscular tissue, and, therefore, the great benefit of iron in weak heart. As regarded digitalis, he had always acted on the principle, that this remedy is efficacious in proportion to the weakness and irritability of the organ, and hence the reason why some had called it a tonic. Owing, however, to the prostration produced by its long continuance, a very prevalent opinion existed that it was a remedy not available in a depressed state of the system; whereas the opposite is the fact when the heart is concerned, a reduced frequency of the pulse being a more correct test of the influence of the medicine on the circulation than the force of the arterial action. Dr. Wilks mentioned an instance where a patient was literally restored to life by its use. The case was that of a woman, who, having long suffered from disease of the heart, had a severe flooding after labour. She was apparently *in articulo mortis;* her limbs were cold; her body in a state of deathly clammy sweat; the face livid; no pulse to be felt at the wrist, and a mere fluttering to be heard when the ear was placed over the region of the heart. Brandy and ether had been given without any good effect, and, as dissolution was every moment expected, it was determined to try digitalis. Half-drachm doses of the tincture were given every hour, and after four doses a reaction had taken place, and after seven doses complete recovery had occurred. The heart, though irregular, was beating with a defined action, and the pulse could again be felt. From this time recovery took place.

It must be remembered, however, that, as Dr. Dickenson has shown, digitalis in large doses checks uterine hemorrhage; but even if the improvement in Dr. Wilks' case were due to this, the principle is probably the same, that digitalis is a tonic, not to the heart alone, but to the (organic) muscular system generally. In Dr. Fuller's recent work on "Diseases of the Chest" will be found some interesting remarks on the action of digitalis. He writes:—"It is a most valuable remedy when given in the treatment of dilatation, and is dangerous only in hypertrophy. Whenever the pulse is feeble and irregular, and more especially when, from any cause, its feebleness and irritability are temporarily increased, digitalis is, of all known remedies, the most useful."

The above views of Dr. Handfield Jones, Dr. Wilks, Dr. Sutton, and Dr. Fuller are not yet generally received by the Profession. The exact reverse is generally held—that digitalis is valuable in hypertrophy and dangerous in dilatation. One of our most distinguished authorities on diseases of the heart, Dr. Walshe, writes:—If the power of the ventricles be seriously impaired, digitalis cannot be given without excessive risk, and had better be altogether avoided."

We require an extensive series of cases with careful observations as to the valves damaged, and as to the result, as regards regurgitation and obstruction (as aortic obstruction, for instance, is of infinitely less consequence than aortic regurgitation), but, more than all, as to the condition of the heart itself as regards hypertrophy, dilatation, and muscular power.—*Medical Times and Gazette, Jan.* 16, 1864, *p.* 60.

DISEASES OF THE ORGANS OF RESPIRATION.

22.—ON THE TREATMENT OF ASTHMA BY THE IODIDE OF POTASSIUM.

By Dr. Hyde Salter, F.R.S., Assistant-Physician to the Charing-Cross Hospital.

[Iodide of potassium has for many years been esteemed a remedy of value in the treatment of asthma. Dr. Salter does not, however, consider that it deserves quite so high a place as has been given it by some of its advocates.]

Sometimes, however, I have seen most striking results attend its use, as the following cases will show:—

E. H., a lady aged thirty, who had suffered from asthma for seven years past, came under my care in September 1863. Her paroxysms were of two kinds—very severe ones, lasting several days, at long intervals; and slight ones, occurring every night, and disturbing her sleep for an hour or two. From these last she had been suffering every night for some weeks when I saw her. Omitting many of the details of her case, I may mention the following as the most relevant:—Diet appears to exercise no influence on her attacks. Damp places agree with her the worst, and she is never well for the first day or two on going to a new place. She is liable to what she calls attacks of renal congestion, in which the urine is very thick and high-coloured, and she thinks that this state of the urine is associated with and produces the asthma. Her father is a martyr to rheumatic gout, quite crippled by it, and has been for years; an uncle died of gout quite young. She has tried an infinity of medicines, and, as far as their effects go, they may be divided into two classes: those which give her slight relief, and those which give her no relief at all. Among the former are—inhaling chloroform, smoking stramonium, smoking various forms of cigarettes, burning nitre paper and inhaling the fumes, ipecacuanha as an emetic, mustard plasters, blisters, chloric ether in thirty minim doses. Among the latter are—strychnine and nux vomica, valerian, lobelia, hot strong coffee, sulphuric ether, Indian hemp. The benefit derived from inhaling chloroform, fumes of nitre paper, from ipecacuanha as an emetic, and from chloric ether, is great at the time, for the smaller attacks, but in each case evanescent.

When I first saw this lady, she was staying at Chertsey, and having the minor attacks every night. I prescribed for her extract of stramonium, and one or two remedies which she had not tried. She called on me again about the 5th of October, and informed me that what I had prescribed for her did not seem to have affected her in any way: she still had the slight attacks every night. She was going away in three days to a place in Surrey, where she had always been bad and had had some of her most violent attacks; indeed, she had never been there without being violently asthmatic the whole time, and she looked forward to her visit with great apprehension. The house to which she was going was built, as she described it, almost in a well—in a place surrounded with water on all sides, and which was rather *wet* than *damp*. I ordered her five grains of iodide of potassium and twenty minims of aromatic spirit of ammonia, in a wineglass of water, three times a day. I saw her husband on the 22nd of October, seventeen days afterwards, and his report was as follows:—She had begun the iodide of potassium a day or two after I had ordered it, and had not had an attack of any kind, severe or light, since. The minor attacks had entirely ceased, and she slept uninterruptedly through the night, a thing she had not done for two or three months: she had gone to the dreaded place, and no attack had occurred—the first time in her life that that had ever happened. Her husband did not know when he had seen her so well. She was daily gaining flesh and strength. The lady herself, with great simplicity, gave the strongest possible testimony to the effect of the remedy by saying, in her written account of herself, that "she had been so well since she had been taking it that she had had no opportunity of trying what its effect would be upon her asthma." To which of course, I replied, that I did not care how long the same result should keep her ignorant of the virtues of the remedy.

It is now nearly a month since she has been taking the iodide, and she still remains perfectly free from her former symptoms. Occurring, as the change did, suddenly, and coincidently with the taking of the medicine, and under the most unfavourable circumstances—that is, when she was going to a place where she had never before escaped severe asthma as long as she was in it, I cannot but attribute the result to the remedy. What will be the effect of leaving it off, and whether on future occasions its results will be equally striking, the future only will show.*

The following case, in which the iodide appeared to be equally beneficial, was under my care during the past autumn:—

T. H., a tall, pallid, spare man, aged sixty-two, had had asthma for six years, and for the last three the attacks had been frequent and very severe. He generally had an attack once a week, and if he escaped a fortnight thought himself very lucky. When I first saw him he had had for some time slight attacks every morning about four o'clock that woke him from sleep, and compelled him to sit up and cough and wheeze for an hour. There was no history of gout in the case; but there was clear evidence of occasional attacks of bronchitis. With regard to treatment, there was the old story: an infinity of remedies had been tried, and, with a single exception, nothing had done him any permanent good. That exception was chloroform, which never failed to give immediate relief. On waking each morning with his usual attack, from half a drachm to a drachm was inhaled. The difficulty of breathing at once subsided, the patient went off into a tranquil sleep, and there was an end of it; whereas, if the chloroform was not given, the dyspnœa would go on increasing, become very tedious, and very likely culminate in a regular attack. But with this single exception, all remedies that had been tried appeared inert. Several things that had not been employed I made a trial of, but with an equally unfavourable result till I tried iodide of potassium. The effect of this was very soon shown. No severe attack occurred after it was commenced, and in a few days the regular morning attacks ceased also. The patient now slept all night without disturbance, and there was no longer any necessity for resorting to the chloroform. This went on for six weeks; the iodide of potassium was then left off. In a few days the asthma began to show itself again, and in a week or two was as bad as ever. The iodide was then resumed, with the same beneficial results as before. I have not heard of this patient now for more than a month; and this very circumstance inclines me to hope that this remedy still keeps his enemy at bay.

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* About a month after writing the above, I heard that this lady had had a severe attack of acute bronchitis from exposure to cold. She was taking the iodide of potassium at the time. The bronchitis was very severe, so that for a day or two her life was in danger; but she had *no asthma*, although on all former occasions on which she had had bronchitis it had induced asthma. On the abatement of the bronchitis, I advised the resumption of the iodide of potassium; and at the time that I last heard from her there had been no reappearance of the asthma, and this was fully two months from the time it was first given.

It should always be borne in mind, in giving iodide of potassium for asthma, that it is often some time before it begins to take effect. I have a patient at the present time under my care who has been taking it for three weeks past in eight grain doses three times day, but it is only during the last week that any decided improvement has taken place in him. He has lost his spasms; the expectoration has very much decreased; and he has ceased to experience an abiding "thickness" and tightness of breathing that he had in the intervals of the attacks, and which never left him. His nurse tells me that whereas before, for months past, whenever he was asleep his breathing was audible and laboured, and accompanied with a slight wheezing, it is now inaudible and tranquil. Yet for the first fortnight this patient derived no apparent benefit whatever from the drug, and was anxious to give it up; now, however, he is convinced of the good it is doing him, and is anxious to continue it. It may be asked, why do I think that the improvement is really to be assigned to a remedy that seems to remain so long inoperative? Why may not the apparent benefit be a coincidence, and the drug be really doing him no good whatever? I think the improvement is the work of the iodide for two reasons. In the first place, from the fixedness of the patient's previous condition for a great length of time, no medicines or any other agencies that were brought to bear upon him making any difference in him. In the second place from this tardiness of the action of the iodide of potassium corresponding with its action in other affections. How long is it, for instance, before it makes any appreciable impression upon a goitre, however complete and satisfactory its results may ultimately be?

I used to think that the benefit derived from iodide of potassium in asthma was entirely due to its beneficial influence in chronic bronchitis, and therefore that the only cases of asthma in which it did any good were cases in which chronic bronchitis and asthma coexisted, and the one was the exciting cause of the other. I am compelled, however, now to abandon that view; for in some of the cases in which its efficacy has been the most striking there has not been a trace of bronchitis.

Another theory that I once held I am also obliged to abandon —namely, that it was of advantage only in those cases in which the asthma was due to a gouty or rheumatic-gouty condition;

and that it was by relieving this condition that it relieved the consequent asthma. In two of the cases that I have related this view would be borne out, for there was evidence of gout in both of them; but in the third there was not a trace. Moreover, I have seen cases of true gouty asthma in which iodide of potassium has been of no service.

Of its ultimate and exact *modus operandi* I can neither offer any explanation nor form any reasonable opinion. I am not, however, the less satisfied of its occasional great value, and of the propriety of its use in any case in which it has not been tried.—*Lancet, Jan.* 23, 1863, *p.* 92.

23.—ON THE USE OF TINCTURE OF LARCH IN CHRONIC BRONCHIAL AFFECTIONS.

By Dr. Headlam Greenhow, Assistant-Physician to the Middlesex Hospital.

[In cases of chronic bronchitis in which copious expectoration indeed continues, but the acute symptoms have subsided, the object of the Physician must be rather to check than to encourage the continuance of the expectoration.]

In patients who have suffered from repeated attacks of bronchitis, this stage of the disease often persists for a lengthened period, and may even ultimately become the habitual condition of the patient. A somewhat analogous condition of the bronchial membrane is frequently met with in cases in which there has been no previous attack of acute or sub-acute bronchitis, but in which the ailment, having begun with a slight increase of bronchial secretion, has gradually merged into a chronic cough attended by expectoration. This latter form of bronchial affection is especially apt to occur in patients of gouty constitution, and also in persons exposed to breathe either the over-dried atmosphere of artificially-warmed and ill-ventilated apartments, or air charged with mechanical, gaseous, or other irritants. In whichever of these forms the chronic flux from the bronchial membrane may present itself, its tendency, even in slight cases, is to impair the general health and vigour of the patient, and to render him more especially liable to suffer from catarrhal attacks, each repetition of which further aggravates, and tends to render permanent the bronchial affection. Various remedies have long been in use for the treatment of this class of cases, especially balsamic medicines—such as balsam of copaiba, ammoniacum, and compound tincture of benzoin. Of these, balsam of copaiba is undoubtedly the most efficacious; but its nauseous taste and smell, for the most part, forbid its employment. Moreover, in many of such cases, these is a feeble condition of the digestive powers in which these balsamic medicines are apt to disagree with the stomach, and a want of general tone and vigour, requiring the administration of tonics. I have now, during some five or six years, been using, with much success, the tincture of larch in the treatment of these forms of bronchial affection. The tincture is made from the inner bark of the larch tree, and its taste is much less unpleasant than that of any of the above-named remedies. I have rarely found it disorder the stomach, and it is capable of being given in combination with tonics, or any other remedies the case may require. I most frequently prescribe it, in hospital practice, in doses of from twenty to thirty minims, in a mixture consisting of tincture of gentian, nitro-muriatic acid, and water, with or without ipecacuanha wine, and either tincture of hyoscyamus or compound tincture of camphor, according to the more or less frequency and severity of the cough. When desirable, the mixture may be rendered more agreeable to the taste by the substitution of syrup of orange peel for tincture of gentian. Of the value of the tincture of larch as a remedy in the large class of cases above described, I entertain no doubt, having tested its efficacy very carefully, and having repeatedly found that patients improved greatly under its use when other medicines had failed. I have in a few cases tried an extract of larch bark, but have found it less decidedly useful than the tincture, which is also the most convenient form for administration. It will be inferred from the tenor of the foregoing observations, that tincture of larch will be found useful only in chronic forms of bronchial disease, attended by considerable expectoration. Its employment is contraindicated in acute bronchitis, or during the intercurrent catarrhal attacks, to which patients suffering from chronic bronchitis of long standing are so liable. Its effect as a remedy is gradually to lessen the amount of expectoration, and with it the cough and dyspnœa, and at the same time to render patients much less subject to catarrhal attacks at particular seasons or changes of weather.—*Medical Times and Gazette, Feb.* 20, 1864, *p.* 200.

24.—BROMIDE OF AMMONIUM IN HOOPING COUGH.

By Dr. George Harley, Physician to University College Hospital.

[Bromide of ammonium has been tried by Dr. Harley for some time in all cases of hooping cough which have come under his care. The two following have occurred recently. We do not suppose that the same success will invariably be found to ensue from its use.]

Oct. 6th. Louisa M., aged nine, is a moderately well-developed child; she is rather pale, and has never been very strong or healthy; has a bad appetite. For the last two months she has been suffering from hooping cough, and hoops two or three times during the day, and once or twice in the night. Ordered nine grains of bromide of ammonium to be taken in half an ounce of water three times a day.

13. Much better; the mother says the girl is "much improved since taking the medicine, and is not like the same child." She lost the hoop after taking five doses of the bromide of ammonium.

John M., aged six, also came to the hospital on the 6th of October, having had hooping cough for two months. He hoops three or four times in the day, and almost every half hour in the night. The cough is not so bad now as it was a fortnight ago. This patient was also ordered to take the bromide of ammonium in six-grain doses three times a day.

13th. The boy lost the hoop after taking the medicine one day, or after four doses; has now only slight bronchitic cough.

20th. Both the above patients visited the hospital to-day, and have now only slight cough; they were ordered some tonic mixture, with cod-liver oil, to improve their general health.

27th. Discharged cured.

[In a letter to the Editor of the Lancet, Dr. Harley remarks that, as a rule, a grain should be given as a dose for every year of the patient's age. He continues :]

To some strongiy-developed children I have given as much as twice the above-named dose. Thus at the present time I have under treatment a child aged three years who is taking six grains, and another aged four years who is taking eight grains of the bromide three times a day. If your correspondent's child is well developed, he may begin by administering one grain three times a day.

All cases of hooping-cough are unfortunately not equally benefited by the remedy ; some pertinaciously resist its action, while others as readily yield to it. The report of some late cases —for the notes of which I am indebted to Mr. Joseph Thompson, one of the pupils at the hospital—presents a striking illustration of the truth of the latter observation; while on a future occasion I purpose furnishing some cases illustrative of the correctness of the former. The difference in the result of these two sets of cases is, I believe, not due to any variation in the effect of the remedy, but to the peculiarity of the cases themselves. This is, however, a point for future discussion.—*Lancet, Jan.* 16, 1864, *pp.* 66, 82.

DISEASES OF THE ORGANS OF DIGESTION.

25.—ON THE TREATMENT OF DIPHTHERIA.

By Dr. William Gibson, Campbelton.

[This paper is founded upon observations made upon fifty-one cases of diphtheria which came under the author's treatment during a period of two years and a half. At the commencement of a case he generally gave a compound jalap powder, with a grain or two of calomel, but any other purgative acts as well. This has the effect of lowering the febrile symptoms and lightening the patient. The diet should be varied, according to the desires of the patient and the circumstances of the case; but, as a rule, it should be something very light and digestible. If there are signs of asthenia present, beef-tea and wine should be given, but never too freely.]

Salines which act on the skin have been recommended at the commencement of the disease, or so long as the febrile symptoms last; but in my practice I never saw them have any decidedly good effect, and their administration was the means of turning away the patient from articles of diet more essential.

The tincture of the perchloride of iron I tried in some cases, and that most perseveringly, but without any apparent advantage; and the giving of it was attended with this effect, that it was soon nauseated by the patients, and this made them nauseate other things more beneficial.

In two cases in the asthenic form, where the urine became albuminous, and when the patients were at the weakest, the carbonate of ammonia, in several grain doses every few hours, was apparently of great benefit.

The chlorate of potash was of no service in my hands; its internal use had no effect over the exudation.

In the great majority of cases, it appears to me almost certain that the continuance of the general symptoms, or the disease, depended very much on the extension of the exudation; that if the latter ceased to spread, the former abated. This took place even though its extension was stopped by treatment; hence the great importance of the local application of those medicines possessing such power; for not only, to all appearance, was the local affection controlled by them, but the general disease as well. The remedies of this nature that I tried, were a solution of nitrate of silver, about a scruple to the ounce of water; the lunar caustic itself; and hydrochloric acid with about one half or one third water. The latter I came to regard with the greatest favour, as being the most effectual. I found it better in using them to limit their application to the exudation, and avoid as much as possible the parts simply inflamed—taking care, however, to see that they were applied freely to the whole extent of it. If after the first application it ceased to spread, the use of the remedy was discontinued; but if it still extended, the remedy was repeated; in this I was not limited by intervals of so many hours, but according as it spread. Some cases were completely controlled by one application, some by two or three, while in others it required to be made repeatedly.

I have seen the occasional use of the same remedies have a good effect when the exudation was declining, and where it had been extensive, and sanio-purulent matter flowing freely from it, by hastening its disappearance, and lessening the discharge.

Gargles I never found of much service. The great majority of the patients were unable to use them, and even those who could, had often a decided aversion to them. After trying them in the first few suitable cases, I abandoned their use, and that without any bad result. I found dusting the throat with powdered alum or tannic acid much easier, and apparently more serviceable; the benefit derived from it was often considerable.

Warm poultices, or stupes by wringing a sponge or piece of flannel cloth out of hot water, and applied round the neck with a dry cloth above, were frequently useful, especially if the glands of the neck were much swollen or pained, or if deglutition was difficult. Their application in such circumstances was often very grateful, and they had the effect of not only affording relief, but of giving one the impression that they helped in stopping the exudation.

Inhaling the steam of hot water, or this medicated with vinegar or strong acetic acid (I found the latter preferred by some patients), when the inflammation or pain in the throat was severe, was also sometimes agreeable, and in several instances gave much relief.

When the exudation had entered the windpipe, some of the above means, such as steaming the throat, warm poultices and stupes, might still be found useful in affording some ease, though of little service in warding off a fatal termination. In the only case of this kind which recovered, I found these very useful. Emetics were also of service in this case, as well as in others which proved fatal, in affording temporary relief. The emetic which I preferred was the sulphate of copper. In some cases the application of mustard poultices to the neck and upper part of the chest, or to the back of the neck and between the shoulders, did good in relieving the attacks of dyspnœa. In one case I tried tracheotomy; but though giving great ease for a few hours, and evidently prolonging life for a brief space, yet the difficulty in breathing soon returned, and the child died in about twelve hours after the operation. In two others it was proposed to have this operation performed, but this was overruled. In another case where liberty was given to perform the operation, but where I was accidentlly prevented from doing it, on a post-mortem examination of this patient (the only one I was allowed to make), the exudation was found not to have passed the larynx, and the trachea quite healthy, showing how favourable the case was for it.

In treating the anasarca and paralysis occasionally followed this disease, in the former I relied on the tincture muriate of iron, time, diet, and nursing. It did not appear to me proper to use either purgatives or diuretics to any extent, so weak were the patients. In all the recovery was tedious, but complete. In the paralysis the only means used was the tonic regimen.—*Glasgow Medical Jounal, Jan.* 1864, *p.* 405.

26.—ON SPURIOUS DIPHTHERIA; ITS NATURE AND TREATMENT.

By G. Stevenson Smith, Esq., Letham, Fife.

Much confusion is often caused, and many hindrances to the advancement of medicine are thrown in the way, by a loose and indiscriminate application of names; it ought, therefore, to be the aim of every one who has the interests of his profession truly at heart, to attain to clear and distinct ideas as to the nature of disease, so that he may at all times think, and judge, and act, with precision. And one way in which we may assist in clearing up matters is, by studying diseases which are allied to each other, and by carefully observing and pointing out the distinguishing characteristics of each.

My object at present is to direct the attention of the profession to an affection which, in many respects, resembles diphtheria, and may be mistaken for it, but which, it will be found, differs essentially from that disease, both in its nature and its results.

During the prevalence of an epidemic, it is usually noticed that there is a strong tendency to a particular form of disease. When cholera prevails, for example, there are always many cases of severe diarrhœa and vomiting, which get well, and the true characteristics of the epidemic affection never become fully developed; in these there is a tendency to cholera, but it would be a misapplication of terms to call them real cholera cases, and so it is, I believe, in epidemics of diphtheria. Throat affections have a tendency to take on this form of disease, and many, many cases which are called diphtheria, and are even treated as diphtheria, are, I am convinced, merely examples of the affection I am about to describe.

How otherwise can we account for the apparent success of one practitioner in his treatment, and the total failure of another!

We hear of one man curing his diphtheria cases with one remedy, while another is equally successful with something totally different; but only let the boasted remedies be applied to a really sèrious case of true diphtheria, the diphtheria which Bretonneau studied so thoroughly, and has so graphically described, and I am convinced they will turn out to be utterly impotent and useless.

It is of importance then to distinguish between the two affections, and I shall now endeavour to sketch the characteristics of spurious diphtheria.

In the course of a recent outbreak of diphtheria, my attention was drawn to a certain class of cases which, while they presented some of the symptoms of that affection, never assumed such a serious nature, or called for such a vigorous plan of treatment, as did those which had previously come under my care.

In the class of cases to which I allude, the patient usually complains first of a curious feeling in the throat, as if a pin were pricking it; there is languor, with pains in the back and legs; and sometimes considerable tenderness on pressure on the outside of the throat, just under the angle of the jaw.

On looking at the throat, the tonsils and uvula are more or less tumefied, according to the severity or mildness of the case, and of an angry red colour, while on their surface small, irregularly shaped, yellowish white spots will be observed.

The spots are evidently of an aphthous nature; there may be only one or two on the tonsil or on the uvula, or they may be so numerous as to give to the soft palate an appearance as if some one had shaken a box of white pepper over it.

However great their number may be, I have observed that their edges do not coalesce, each spot is isolated. They never look excavated, but seem as if they just floated on the mucus which moistens the throat.

The appearance of the tongue usually indicates derangement of the digestive system, and the pulse is smaller and more frequent than in health.

The treatment of spurious diphtheria is exceedingly simple—a mild aperient, the tincture of the muriate of iron, in doses of ten or fifteen drops thrice a-day, with a simple gargle of chlorine water, will certainly and speedily cure the throat affection. There may be a good deal of prostration and muscular debility after an attack of this disease, but a liberal diet, and the use of stimulants, if necessary, will soon restore the patient to health.

Aphorisms.—Spurious diphtheria, so far as my observation has extended, never proves fatal. Though accompanied with debility, I have never seen it followed by paralysis or albuminuria; the tonsils sometimes suppurate after an attack. A patient who has suffered from this affection may subsequently be attacked with true diphtheria.

In true diphtheria, gargles are of very little use till the patient begins to recover, but in the disease under consideration their use is always followed by the greatest benefit from the very first. In spurious diphtheria the use of caustic is not required. I do not know how to account for it, but this affection seems to be most prevalent amongst young females.—*Edinburgh Medical Journal, November* 1863, *p.* 452.

DISEASES OF THE URINARY ORGANS.

27.—SIMPLE METHOD OF ESTIMATING THE ACIDITY OR ALKALINITY OF URINE.

By M. Donovan, Esq., M.R.I.A., Honorary Member of the Philadelphia College of Pharmacy.

During the existence of the uric acid diathesis the exhibition of alkaline substances is a part of the treatment generally resorted to. The *liquor potassæ* is much used, but is liable to the objection, that on account of its powerful agency on the coats of the stomach, an efficient dose can scarcely be ventured on. The bicarbonate of potash is therefore preferable. This form of the alkali will react upon any acid present in the system, no matter whether it be free uric, phosphoric, sulphuric, muriatic, or lactic acid. The agency of any one of the four last is to be feared when there is an abnormal quantity of urate of ammonia present, as the latter will be decomposed, and the uric acid thus eliminated may aggregate into the form of gravel or calculus. Not only will the acids present in the stomach produce this effect, but even the most slightly acidulated beverages or esculents; whence it is that during the prevalence of the uric acid diathesis all acid wines, acid drinks, acids of all kinds, and acidulous fruits are to be avoided. I have known the urine of a person to be so highly charged with urate of ammonia that a few spoonfuls of hard porter, or the eating of an apple, would next day cause the excretion of two or three small crystals of uric acid. No wonder that indulgence in acidulous or acescent articles of food should develop uric calculus during the prevalence of the uric acid diathesis. In the case of the person above alluded to, it was ascertained that if a portion of bicarbonate of potash were swallowed immediately after or before the ingestion of a moderate quantity of acidulous wine, porter, or fruit, no crystalline substance was passed with the urine.

Although alkalies enter the circulation and neutralize acidity wherever generated in the body, it is well known that, if taken in greater quantity than necessary for attaining the point of neutrality, they will produce as bad effects as any acid would have done by precipitating the earthy salts of the urine, and thus giving origin to a calculus, although of a different kind.

Sir Benjamin Brodie observes that, as too little alkali will do proportionately little service, and too much may thus do positive injury, watchfulness is indispensable, while alkalies are exhibited, during the prevalence of the uric acid diathesis. He recommends that the urine be frequently tested by litmus and turmeric papers; the alkali, he warns, is never to be urged so far as to neutralize the acid entirely. The chief defect in this mode of ascertaining the state of the urine is that the test papers do not afford information about the daily increase or diminution of acidity or alkalinity. The intensity or depth of the change of hue will scarcely be recognizable until the critical disappearance of all acidity or alkalinity suddenly occur, and yet it is from precise information as to the approach of this period that the practitioner is to learn whether his treatment be doing injury or service.

To afford the requisite information, I suggest the following method, the·application of which requires but little skill, and is so simple that it may be entrusted to the management of the patient. The apparatus necessary consists of two equal cylindrical test glasses, each about six inches long and nine-tenths of an inch internal diameter. One of these is to be graduated in the following manner:—Pour into it two ounces measure of water, and mark the height at which the water stands with a sharp file. Divide the space between the bottom and the marked line into forty equal parts, which we may call degrees. They are to be engraved, and every tenth degree numbered, the lowest being 1, and the highest 40. The other test glass need not be graduated.

A normal tincture of litmus should be kept in readiness, made by infusing 120 grains of fine powder of best litmus in a mixture of an ounce and a half of water with half an ounce of rectified spirit for a day or two with frequent agitation, and then pouring off the clear tincture.

A normal alkaline solution is to formed by dissolving 160 grains of air-dried bicarbonate of potash in eight ounces measure of *distilled* water. This solution will hold dissolved one grain in the quantity which each degree or division of the test glass is capable of containing. Any other ratio of alkali to water would answer, but not so conveniently for the test glass and its graduation here proposed.

Process.—Mix about half a drachm of the tincture of litmus with about an ounce and a half of distilled water. Pour this purple-tinged water into the graduated test glass until it rise exactly to the tenth degree; and into the same glass pour the urine to be tried, until the total liquid rise to the twentieth degree. The liquor will now perhaps be brilliantly reddened. Pour the whole into the ungraduated glass; add to it twelve or fifteen grains of the same crystals of bicarbonate of potash, and let them dissolve. The red colour will, by this excess of alkali, be restored to its original purple hue. Into the graduated glass pour the same quantity of the remaining tinged water, and the same quantity of urine as before. Now add, by small quantities, as much of the normal alkaline solution (drop by drop as the colour approaches that of the liquid in the ungraduated glass) until the tint in both glasses be exactly the same, shaking the mixture well after each addition. The column of liquor in the ungraduated glass must be kept constantly as high as that in the graduated one by additions of distilled water, in order to ensure similarity of hue by similarity of dilution. The two glasses must then be placed, very near each other, in such a situation that, when the slight effervescence (if there be any) is over, they may be viewed by transmitted light from a bright sky. The number of divisions or degrees to which the liquor in the graduated glass has been increased, by the normal alkaline solution added, will indicate the number of grains of bicarbonate of potash that are the equivalent of the acid or acids contained in the quantity of urine tested, one grain for each degree; and by daily trials made in this manner certain information will be gained of the increase or diminution of acidity, and of the propriety of increasing or diminishing the dose of the alkaline remedies prescribed. By the same means a patient at a distance can communicate to his physician his exact situation, by informing him how many grains to the ounce his instrument indicates, twenty divisions being equal to an ounce.

Commercial bicarbonate of potash in *crystals* is sufficiently pure for the purpose. When air-dried it is unalterable in the air, and will always afford a solution of the same alkaline strength from the same proportions.

It is obvious that this apparatus may be used for testing alkalinity as well as acidity, by an obvious change of means. Thus: make distilled water purple-blue as before; pour it into the marked tube up to the tenth degree; then pour in the alkaline urine until it reach the twentieth degree. Transfer the whole into the ungraduated tube, and add a slight excess of the normal dilute acetic acid hereafter described; the colour changes to red. Repeat the same process with coloured water up to the tenth degree, and the alkaline urine to the twentieth; add the normal acetic acid until the red tint of both liquors be the same, keeping up the level in the ungraduated tube, with distilled water, to the same height as that of the liquid in the graduated tube. The quantity of dilute acetic acid added is the index of alkalinity.

The exact degree of acidity of the normal acetic acid is of little moment, provided it be always the same for the same set of experiments. The more dilute it is, the more sensitive it will be as an index of alkalinity. In general the acidity should not exceed that which is barely discoverable by the taste.—*Dublin Medical Press, March* 16, 1864, *p.* 268.

28.—ON THE URINE.

By Dr. George Harley, Professor in University College, and Assistant-Physician to University College Hospital.

[Urine is nothing more nor less than a collection of the liquid and solid effete products of the frame, and its composition fluctuates in exact proportion as the processes of life fluctuate. Dr. Harley considers that the study of the urine has not done nearly as much as it might have done, nor as it ought to have done, owing to its votaries having always been too eager to profit by its pathology ere they had mastered the first rudiments of its physiology. Dr. Harley thus divides the urines of mammalia into the natural division of—the solid and the liquid urines. The urine is solid in all animals devoid of a urinary bladder, and in which the ureters open into the rectum. But this solid urine which occurs in most reptiles, in birds, and in insects, differs only from liquid urine in the absence of water.]

The liquid urines are found throughout the whole class *Mammalia*, and present three such well-marked varieties, both as regards physical appearance and chemical composition, that it may be said they naturally divide themselves, according to the species of animal, into the three separate groups of carnivorous, herbivorous, and omnivorous urines.

The urine of the first class (carnivora) is characterised as being a clear, transparent, light-coloured liquid, possessing an acid reaction, and rarely depositing anything on cooling.

The urine of the second class (herbivora), on the other hand, is recognised as being a dark coloured liquid, with a strongly alkaline reaction, and depositing a copious sediment within twenty-four hours after being passed.

The urine of the third class (omnivora) lies, as it were, between the two. It is clear, slightly acid, somewhat darker in colour than that of the carnivora, but considerably paler than that of the herbivora, and only occasionally deposits a sediment on cooling.

In chemical constitution these urines present as marked features of difference as they do in physical appearance. Thus, for example, while uric acid, a beautiful crystalline body, exists in the urine of the carnivora, it is entirely wanting in that of the herbivora.

The urine of the herbivora, on the other hand, contains, in its stead, a quantity of a totally distinct organic acid, namely, hippuric, which not only differs in chemical composition, but also in crystalline form. The urine of the omnivora is again found to stand between the two, and contains a portion of uric as well as hippuric acid.

Even in the nature of their inorganic ingredients, these urines differ materially; for, while both alkaline and earthy phosphates are abundant in the urine of the carnivora, they are entirely absent from that of the herbivora. Their place being there supplied by alkaline and earthy carbonates. Here the urine of the omnivora still occupies the medium position, and claims a share of both.

Having now seen that the urine varies in the different species of animals, we are in a measure prepared to consider how and why it should vary at different times in the same animal. The variations both as regards quantity and quality we shall find to depend upon the state of the body, the kind of food and drink, the amount of exercise, the climate, and an infinite number of minor causes, the influence of which will afterwards appear.—*Medical Times and Gazette, Jan.* 2, 1864, *p.* 4.

29.—ON UREA.

By Dr. George Harley, Professor in University College, and Assistant-Physician to University College Hospital.

Urea is to be found in the urine of all animals, but exists in greatest quantity in that of the mammalia. It never occurs as a urinary deposit, in consequence of its great solubility in water, being, in fact, deliquescent.

Before entering upon the physiological, and pathological relations of urea, it will be necessary for me first to say a few words regarding its appearance and properties.

The urea of commerce is a white, semi-transparent, crystalline body, with a bitter taste. When heated it melts, burns, and gives off ammonia. It is soluble in alcohol, as well as in water, but nearly insoluble in ether. It is readily decomposed by strong mineral acids, and by hydrated alkalies. With chlorine it yields carbonic, and hydrochloric acids. Nitrogen gas being at the same time set free. In contact with animal ferments urea takes up two equivalents of water, and is converted into two parts of carbonate of ammonia.

$$C_2H_4N_2O_2 + 2HO = 2(CO_2NH_3).$$

Hence the peculiar ammoniacal odour of putrid urine.

Urea readily combines with nitric acid to form the most important of its salts:—

The Nitrate of Urea ($UrNO_5HO$), which forms, when rapidly crystallised, flat, shining, rhomboidal plates; when slowly crystallised, fine prisms. It is very soluble in both water and alcohol, but only sparingly soluble when they contain an excess of free nitric acid.

We have here three specimens of urine—man's, horse's, and dog's—representing the three subdivisions of the animal kingdom into omnivora, herbivora, and carnivora; the first two urines have been concentrated to about half their normal bulk, the third is left in its natural state, it being by far the richest in urea. To each I now add equal parts of strong nitric acid, when instantly you perceive they begin to crystallize, and before many minutes elapse each of the liquids will have become one compact mass of shining crystals; I shall be able to invert the glasses without danger of spilling their contents. You observe the crystals are coloured. Those from the dog's urine only slightly, those from the horse's very deeply. This is not owing to the nitrate of urea being a coloured salt, but simply because it happens to be deposited in a coloured liquid, part of the colouring matter of which has united with it; just as sugar—a white substance—when crystallised in a solution of carmine, yields red crystals. The higher coloured the urine, therefore, the deeper coloured the crystals; the paler the urine the whiter the crystals.

With oxalic acid urea forms an oxalate (UrC_2O_3HO) which crystallises in prisms and quadrilateal tables, sometimes separate, sometimes adhering together in groups. This salt is freely soluble in hot, but sparingly soluble in cold, water. Hence it is that we occasionally, though very rarely, detect crystals of oxalate of urea in concentrated human urine.

Physiological Relations of Urea.—Urea was first discovered in human urine, and it was for a long time thought to be one of the special products of the animal world. Modern chemistry having, however, succeeded in preparing it artificially from a variety of substances, among which may be mentioned the cyanates of lead and silver; together with the fact of its being also formed by the union of cyanic acid and ammonia—

$$\overbrace{C_2NO,HO}^{\text{Cyanic acid.}} + \overbrace{NH_3}^{\text{Ammonia.}} = \overbrace{C_2H_4N_2O_2}^{\text{Urea.}}$$

many persons are inclined to regard it as merely the cyanate of ammonia.

Urea, though found in the urine, is not formed in the kidneys, but only excreted by them from the blood, where it has been detected by Garrod, myself, and others. Picard found 0·016 per cent. in normal human blood, and on one occasion I found as much as 0·320 per cent. In the defibrinated blood of a sheep I found 0·559, and in that of the dog as much as 0·747 per cent. In a normal state the blood of the renal artery contains twice as much urea as that of the renal vein (Picard), but in cases of its retention, both vessels contain about an equal quantity. Wurtz obtained urea from the healthy chyle, and lymph of the ox, horse, and dog. It has been found in the humours of the human eye, and is one of the ordinary constituents of the amnoitic fluid of all mammalia; but, as I said before, it owes its presence in the latter situation to the urine excreted by the fœtus while still in utero.

It has been asserted by some (Robin and Verdeil) that no urea is to be found in the urine of very young children; this however, is an error, as I have detected urea in urine taken from the bladder of a still-born child, and in that of an infant aged eight weeks I found as much as five grammes per thousand. In fact, from the subjoined table it will be seen that children actually excrete relatively more urea than adults; and males absolutely as well as relatively more than females, at all periods of life.

	Quantity of Urea in the 24 hours' Urine.			
Sex and age.	Absolute amount.		Relative proportion per lb. of bodily wght.	
	grammes.	grains.	grmes.	grains.
Boy aged 18 months	8 to 12	= 124·0 to 186·0	0·40	6·2
Girl ,, ,, .	6 to 9	= 93·0 to 139·5	0·35	5·4
Man aged 27 ” .	25 to 35	= 387·5 to 542·5	0·25	3·8
Woman ,, .	20 to 30	= 310·0 to 465·0	0·20	3·1

In this table it will be seen that the amount of urea daily excreted is liable considerable variations. Before attempting therefore, to draw any conclusions from the changes observed in its amount during disease we must make ourselves thoroughly acquainted with the causes upon which the normal fluctuations depend, and in order to do this the more effectually we must first inquire into its normal source.

No urea has as yet been detected in any of the human solids, except what might readily be accounted for by the presence in them of blood. Its source seems to be two-fold; firstly, the disintegration of the tissues; and, secondly, the excess of nitrogenised food absorbed into the circulation.

The effect of food on the daily elimination of urea is well marked in the results obtained by Von Franque while experimenting on himself:—

UREA.

Diet.	In 24 hours.		Per hour.		Per lb. of bodily weight.	
	grmes.	grains.	grmes.	grains.	grmes.	grains.
Animal (3½ lbs. of flesh)	92	= 1416·0	3·86	= 59·8	0·53	= 8·2
Mixed	37	= 573·5	1·58	= 24·4	0·21	= 3·2
Vegetable	28	= 434·0	1·08	= 16·7	0·15	= 2·3
Non-nitrogenous	16	= 248·0	0·69	= 10·6	0·09	= 1·3

It is here seen that on an animal a much greater quantity of urea is excreted than on a mixed diet, and least of all after non-nitrogenised food has been taken. These results are in accordance with those of all other observers, except that the total quantities are greater.

Some suppose that urea comes in no case directly from the excess of absorbed food, but only indirectly from that portion of it which has undergone metamorphosis in the tissues (Bischoff, Voit, Parkes). If such be in reality the case, it appears to me very extraordinary that when living on a purely animal diet a person should excrete a larger proportion of urea than when living on mixed food, the mixed being the most nourishing diet of the two. Again, if their views be correct, why is it that exercise, which is well known greatly to increase the metamorphosis of the tissues, is not found materially to affect the daily elimination of urea? Thus, Von Franque excreted in twenty-fours hours:—

Diet and exercise.	Urea in 24 hours. grammes.	grains.	For every lb. weight. grammes.	grains.
Mixed, with moderate exercise	37·983	= 588·73	0·218	= 3·379
Mixed, with much exercise	37·877	= 587·09	0·217	= 3·363

Actually less urea after much exercise than he did on his ordinary average exercise. On the other hand Dr. Ed. Smith found that prisoners excrete 36 grains more of urea when at hard work than when doing nothing, which, he adds, is 19 grains above what they pass on light labour.

Again, we find that Bischoff in his experiments on dogs observed that they passed most urea when fed on eggs, muscle, and gelatinous substances, and that it increased in proportion to the quantity of food given. These facts all tend to the conclusion that a portion of the urea excreted by the kidneys comes from the excess of food, independent of the metamorphosis of tissue. That all the eliminated urea does not come from this source no one seeks to affirm, for there is abundant evidence against such a supposition. For example, starve an animal as you may the urea will never entirely disappear from the urine. It can only be reduced to a certain point and no further. Schmidt, Frerichs, Bischoff, and myself have observed this in dogs, and Lassaigne, Scherer, Von Franque, and others have made similar observations on the human urine. The last-named gentleman starved himself during forty hours, and still found 19·35 grammes (299·9 grains) of urea in his twenty-four hours' urine. On experimenting on the effects of tartrate of antimony as a slow poison, I found that the urine of dogs, reduced to perfect skeletons, contained such an amount of urea that it crystallised on the simple addition of nitric acid without any concentration whatever.

The amount of urea passed by an animal during starvation is greater, if he has been previously well fed, than if he has only received a sufficiency of food to prevent his losing weight (Bischoff), which fact probably accounts for Dr. Von Franque passing so much during the time he was without food.

Prout thought that the urea came chiefly from the gelatinous tissues, and Von Franque thinks it comes chiefly from the muscles, but neither of these tissues has been found to contain urea. It is true that Städeler and Frerichs found a considerable quantity of urea in the muscles of cartilaginous fishes; but that proves little, seeing that they detected it in almost all the organs. My idea is, that *urea is not the special product of any one particular tissue or organ, but the united product of all nitrogenised effete matter.* Bechamp and Picard assert that they have converted albumen and other azotised substances into urea by slow combustion with permanganate of potash, and although subsequent experimenters have not been equally successful, we must accept their statement as a fact until the contrary is proven.

Part of the urea may also be formed in the blood by the decomposition of uric acid. This is rendered highly probable, for two reasons—Firstly, Neubauer found on giving rabbits from two to three grammes (31 to 46½ grains) of uric acid with their food the quantity of urea excreted in the twenty-four hours was augmented from 2·1 to 4·2 grammes (32·5 to 65·1 grains); and, secondly, when uric acid is acted upon by permanganate of potash it is transformed into several substances, one of which is urea.

We have now to consider how the elimination of urea is influenced by other than animal foods. In the first place, water increases it. The more water we drink, *cæteris paribus*, the more urea we excrete, most probably for the reasons already given when speaking of the influence of water on the urinary solids. Common salt also produces an augmentation in the excretion of urea (Böcker, Bischoff). Coffee has an analogous effect. I found this to be very decidedly the case in one of my friends, Baron G., aged fifty-seven. The daily average of urea excreted by him was 17·28 grammes (267·84 grains), whereas after he had restricted himself to nothing but coffee (without milk or sugar) and dry bread during forty hours he passed 25·3 grammes (382·25 grains) of urea per twenty-four hours.

This diet had an equally striking effect on the amount of water eliminated. His usual average was 1290 cubic centrimetres (41 oz.), and on the day in question it rose to 1895 cubic centimetres (60 oz.) per twenty-four hours. This result is contrary to the observations of Böcker and Hammond, who found, on taking an excess of coffee with their ordinary diet, that the daily excretion of urea diminished. But this probably arose from their taking the coffee in the usual way, sweetened with sugar, for there are certain foods which greatly diminish the elimination of urea, and among them sugar holds the foremost rank (Böcker, Bischoff, and Voit). Starch has also a marked effect in diminishing the amount of urea, and I believe that fat may be placed in the same category, for notwithstanding the statements made to the contrary (Böcker and Bischoff), the results of Botkin's experiments on dogs are conclusive regarding the decided diminution of urea which takes place when fatty matters are added to the food. This opinion is further strengthened by the observations of Beneke, who found that cod-liver oil lessens the amount of urea passed by the human subject. In fact, my idea is, that all nitrogenised foods augment, and all non-nitrogenised foods diminish, the amount of urea eliminated by the kidneys. Even gelatine, the nitrogenised substance which of all others is supposed to possess the least nourishing properties, has a decided effect in increasing the daily excretion of urea.

I have entered thus fully into the physiology of urea in consequence of being aware that unless the influence of diet, age, and sex be borne in mind while studying its fluctuations during disease, our labours will be unattended with any practical benefit; while, on the other hand, if these factors are duly considered, we cannot fail to reap important advantage from the knowledge thus acquired, for just as we have seen that its amount in normal urine represents, to a certain extent, the wear and tear of the frame, and is consequently an index to the health of the individual, so also in disease it furnishes us with precisely similar and even more important data; for the amount of urea in the urine enables us not only to judge of the tissue consumption, and, through it, of the severity of the affection, but is at the same time a direct clue to treatment, showing us whether our remedies should be directed to the restraining or encouraging tissue metamorphosis.—*Medical Times and Gazette, April* 16, 1864, *p.* 418.

30.—A SUMMARY OF THE PROPER DIET IN DIABETES.

By Dr. Edward Smith, F.R.S., Assistant-Physician to the Hospital for Consumption and Diseases of the Chest, Brompton.

1. *Fluids.*—To be limited by degrees daily until they shall not exceed five pounds and a half in both fluid and solid food. Of this quantity two to three pints should consist of new or skimmed milk, and one pint, or less, of tea. In the cold season and at night they should always be given when hot. Of all alcohols brandy is the best, and may be given with water only, or added to milk, or beat up with egg and milk, and given several times daily. No fluid should be given in greater quantity than half a pint at a time, and when milk is reduced in volume by cooking, the daily quantity of fluid must be made up by an additional supply of the same or other fluid.

2. *Solids.*—Dr. Prout's combination of eggs and milk (with sharps substituted for bran) is excellent. Four ounces of sharps and 4 oz. of peas, beans, or lentils may be made into bread or pudding, with milk, or into omelettes with eggs and herbs. Eggs and gelatin may be given when starchy food cannot be altogether intermitted. Eggs, gelatin, cheese, gluten bread, meat, fat, and oils may be given as largely as they can be digested. The free use of salad oil should be urged, whether in the cooking of fish or flesh, or in the use of water-cress as a salad, or drank alone, so that several ounces may, if possible, be consumed daily; but as there are in all persons preferences and dislikes in reference to particular fats, that kind—whether butter, suet, oil, or fat of meat—should be allowed which is the most agreeable. Four oz. of sharps, 3 oz. of wheaten flour, 5 oz. of peas, 1 ℔. of meat, 2 oz. of cheese, 2 pints of milk, and 3 eggs, will afford more than about 13 oz. of carbon and 1 oz. of nitrogen daily.—*Lancet, Feb.* 6, 1864, *p.* 149.

SURGERY.

DISEASES OF THE BONES AND JOINTS, ETC.

31.—ON A NEW OPERATION FOR OBTAINING UNION OF AN UNUNITED FRACTURE.

By E. R. Bickersteth, Esq., Surgeon to the Liverpool Royal Infirmary.

In bringing this subject before the attention of the society, the author proposed to mention some cases that had occurred in his practice to show the successive steps by which he arrived at the process in question. He had frequently tried, in vain, friction, acupuncture, and subcutaneous division; and though resections of the ends of the bone had been successful in some instances, it was a proceeding involving a considerable risk to life. Dieffenbach's method had proved to be more successful; but this operation, though conducive to the formation of new bone, in no way provided for what was of paramount importance—viz., absolute immobility of the opposing fragments. The large external wound and injury done to the soft parts in introducing the ivory pegs were also objections to this operation. Recognising the happy influence of Dieffenbach's plan of exciting ossific deposit, and at the same time feeling the importance of keeping the ends of the bone in a condition of absolute immobility, the author was induced to try a modification of the operation; and in the case of a man admitted under his care at the Liverpool Royal Infirmary, with an ununited fracture of the radius, he drilled a hole through the ends of both fragments, and, passing a stout wire through it, secured the bone in perfect apposition. Union took place in seven or eight weeks, but on endeavouring to remove the wire, so much traction was necessary that it caused the facture again to be ununited. This difficulty of removing the wire induced the author to think of some other plan not open to this objection; and in the case of a man with an ununited fracture of the thigh, by means of a common Archimedean drill, he bored two holes in such directions that each passed obliquely through both ends of the fractured bone, and into each introduced a steel rod with a screw at the end. To do this it was necessary to make an incision three inches in length. Much constitutional disturbance folextreme obliquity of the fracture, the pillow, bound firmly round the leg by three or four straps, effects perfect union without over-lapping, and is by far the most simple and easy method of applying the required lateral pressure to the limb with which I am acquainted. It produces neither abrasion nor irritation of the skin, and no discomfort from undue pressure. —*Lancet, Jan.* 9, 1864, *p.* 31.

33.—ON RESECTION OF THE KNEE-JOINT.

By FREDERICK LE GROS CLARK, Esq., Surgeon to St. Thomas' Hospital.

[Mr. Le Gros Clark has excised the knee-joint in seven cases altogether. He observes :]

A remark was once made to me by a surgeon, whose opinions are entitled to the highest respect, that although he regarded the operation of resection of joint as an important acquisition to conservative surgery, as thereby amputation was often superseded, yet he feared that not a few joints were subjected to excision which would otherwise have got well by patient waiting and rest. Whether we may be disposed or not to admit the accuracy of this remark, there is no doubt that it was prompted by a just estimate of the difficulties which involve the cases as a class; for it cannot on the one hand be considered a triumph of conservative surgery to sacrifice a joint which might have been saved, albeit the operation is successful *per se*; nor, on the other hand, is justifiable to risk life by attempting to save a limb, without any reasonable prospect of a permanent relief from a minor operation. There is no doubt that the earlier excision is performed where there is disorganisation in a joint, the better chance there is of the operation proving successful; at the same time it is important that the possible restoration of the joint, or supension of disease, should not be anticipated by too early an operation; for spontaneous recovery with a stiff joint is preferable to recovery with the superadded risk of a serious operation.

With such difficulties surrounding the subject, conjoined with the obscurity which involves these diseases as regards the actual condition of the textures implicated, it is evident that each case must stand more or less on its own merits, and that familiar aquaintance with joint disease can only justify a surgeon in determining the time and character of the operation. Yet there are some general rules which may be observed in determining these questions.

The antecedent history and duration of the disease will be taken into consideration. It may be that the interior of a joint

—and I refer more especially to the knee-joint—may have been the original seat of morbid action, or the joint may have been invaded by the extension of disease from without, such as caries or necrosis of the cancellous texture consequent on injury or ostitis from cold. I do not, however, attach much importance to these distinctions; if there is disorganisation of a joint, it does not import much whether the morbid changes are primary or secondary, but I think we may fairly look for a more favourable issue where the primary disease was accidental and occurring in a constitution uncontaminated by scrofula or other disease, the presence of which would impair the reparative effort. I think this point is illustrated in the cases on which I have operated. The duration of the disease is important, as it bears on the extension of mischief, in the form of abscess or otherwise, to neighbouring textures, and in the depressing effect it has had on the vital power of the patient. For, no doubt, some patients would rally after amputation, in whom we could not reasonably anticipate a favourable result after excision. As regards age, all the cases which have come under my care have been in comparatively young persons, which, I suppose, is not peculiar. I am, therefore, not prepared to say how far this element in the consideration of a case should influence the surgeon in his election between amputation and resection. The actual extent of the disease has an important bearing on the decision. It is not so much a question of how far the joint itself is disorganised, as whether the disease is limited to the articular extremities of the bones involved. The course and termination of superficial abscesses if such exist, should, therefore, be ascertained; and if, thereby, the presence of advanced disease in the bones, at some distance from the joint, is discovered, it would probably be a justification for amputation rather than exision. In doubtful cases an exploration of the parts would lead to a decision as to the proper method of completing the operation. Under all circumstances, after excision, the sawn extremities of the bones should be carefully examined, to ascertain that they are healthy, free from caries, and holding no sequestrum.

I have never had any serious hemorrhage in resection of the knee, and generally the bleeding is trifling compared with what might have been anticipated. The shock, too, is not usually severe; indeed, sometimes very moderate, and followed by moderate and healthy reaction. Pyæmia is said to be destructive in many fatal cases: of this I have no experience.

The operation itself is simple. I prefer a semilunar incision, extending from condyle to condyle, and across the ligament of the patella. The horns of this incision should extend far enough back to secure a free opening of the joint. The bow-saw of

Mr. Butcher is by far the best instrument for removing the diseased surface of the bones; it is admirably adapted for this, as for many other purposes. I prefer making the section of the tibia from behind forwards, and generally, if practicable, remove a slice of the femur corresponding to the convexity of its surface. I have found no inconvenience result from this practice, and the limb is thereby curtailed of less of its length. The patella I have always left, after removing its surface with the saw. To the careful removal, by dissection, of all diseased tissue within reach, I attach great importance.

The limb should, if possible, be placed in its permanent position at once, a suitable splint, with cleanliness and perfect rest, being essential. The flap being accurately adapted with metallic sutures, needs no other support; and any dressing, beyond a spirit-lotion on a thin rag, if grateful to the patient, is superfluous. Osseous union is probably, in many cases, deferred for a considerable period. In the second case to which I have referred, the union was evidently only fibrous, though the limb seemed sound, and well able to support the superincumbent weight before it became flexed. Where there is any doubt, artificial support should be afforded for some time after the patient is permitted the use of the limb. This case also illustrates the arrested development of the limb in a growing boy; but we still want more extended and accurate statistics on this point before a decided opinion can be expressed as to how far this risk should influence our practice in the case of the knee-joint; for arrested growth, resulting from excision in a very young child, might leave the subject of it even a more helpless cripple in after life than amputation. There is no doubt that the opinion expressed by Dr. Humphry is correct, viz., that, in most instances, if the line of union between the epiphysis and shaft is not interfered with, the growth of the limb will not be seriously impeded. But this rule is not without exception, as proved in the second case referred to by me.—*Medical Times and Gazette, March* 26, 1864, *p.* 335.

34.—ON THE REDUCTION OF DISLOCATION OF THE SHOULDERS BY SLOW MANŒUVRES.

By M. Alphonse Salmon.

M. Salmon's plan consists in eluding muscular resistance by the employment of slow and gentle procedures, leading the patient to believe that he is rather engaged in examining the nature of the accident than rectifying the displacement. He is laid flat on his back, and so much on the edge of the bed that the half of the body corresponding to the injured side projects beyond, and is, so to say, suspended outside the bed. Evèn in health, such a position prevents persons exercising extensive movements with the arm without making great effort. An assistant stands beside the bed, to give the patient the idea of preventing his falling, but whose principal object it is really to leave him to make every effort with his sound side, for the purpose of securing for himself a solid position on the bed. The limb having been in the meantime carefully supported, so as to avoid all pain, the surgeon, standing by its side, gently grasps the hand and forearm, and draws the extremity from the trunk with extreme slowness, stopping whenever the patient manifests the slightest suffering, gently kneading the muscles situated around the point of the shoulder, frequently asking the patient whether he is giving him pain, and leading him to believe that the examination will be the more easily made in proportion as the pain is slight. All this may occupy some ten or fifteen minutes, during which the arm becomes, little by little, separated from the trunk, and gradually raised until it is parallel with the axis of the body. In order to effect the reduction, the surgeon gently transfers the limb to the care of an assistant, enjoining him to retain it without any effort in the position given to it, and places himself on the inner side of the limb, opposite to the axilla, as if he wished to explore the cavity of the joint. He now grasps the shoulder with the four fingers of each hand, joining them above the acromion, so as to render the scapula immovable. He next carries the two thumbs gently on to the head of the humerus, situated in the axilla, and, by exerting some pressure upon it—aided if necessary by slight extension made by the assistant who supports the limb—easily causes it to slip into the glenoïd cavity. The arm is then brought to the side, and, to the great astonishment of the patient, who generally has not experienced the slightest pain, the operation is at an end.

M. Salmon adduces six cases in proof of the simplicity and efficacy of the procedure, and exhibiting its superiority to those more forcible means commonly used, which cause much suffering, and often defeat their own object.—*British and Foreign Medico-Chirurgical Review, Jan.* 1864, *p.* 265.

35.—ON AMPUTATION OF THE THIGH BY TEALE'S RECTANGULAR FLAPS.

By Dr. George H. Porter, Surgeon to the Meath Hospital and County of Dublin Infirmary.

It is often a matter of extreme difficulty, in disease of a joint, to ascertain, accurately, the full extent to which the bones entering into its formation are damaged. When sinuous openings have formed about the part it is comparatively easy to discover the amount of injury by means of a probe passed through those apertures; but when such do not exist the uncertainty is much augmented. Formerly, I believe, very many limbs were amputated in consequence of the difficulty just mentioned, which would now be saved by resection. Fortunately the success of cutting out the diseased portion causes us no longer to look upon the loss of the limb as requisite; and, on the other hand, the free opening of an articulation to examine its condition in no way interferes with subsequent amputation if too extensive destruction be found within. The case I am about to mention is one which very strikingly manifests the great necessity of ascertaining, beforehand, the state of the ends of the bones, proving that a large amount of disease may be present, when, to all appearances, it was of a limited degree; and that laying the joint open for inspection does not, in any way, prevent further operation, but, on the contrary, would rather suggest exploration, in all doubtful cases, prior to the removal of the limb.

M. S., aged nineteen years, was admitted into Meath Hospital February 11th, 1863. She gave the following history of her case :—When thirteen years of age, while running on an errand, she fell to the pavement on her left knee, and a fortnight afterwards the joint began to swell and become painful. She then gained admission into one of the hospitals of this city, but remained only a few days under treatment. At this period she was lame from stiffness and pain in the knee; but was able to walk about until May, 1862, when, becoming much worse, she sought admission into the Meath Hospital, where she remained nearly five months, undergoing treatment calculated to bring about an anchylosed condition of the articulation. All this time her general health did not suffer to any marked extent. On the contrary, she looked well and fat; and, feeling relieved, left the institution for the country, a strong leather case or mould having been previously placed round the joint. In this state she moved about on crutches, and occasionally visited the hospital, as an extern patient, up to a week before her last admission, when she was attacked with swelling, excruciating pain in the knee, and frequent startings. On examination I found the joint slightly flexed, but much enlarged, measuring two and a half inches in circumference more than the sound one; it had a globular shape; the sulci on each side of the patella were obliterated, and exquisite tenderness was evinced when it was touched, particularly at the inferior and inner side of the articulation. The lower end of the femur did not appear enlarged; nor did the upper extremity of the tibia seem altered, but it was slightly displaced backwards, and could be moved a little in every direction, on the end of the thigh bone, showing that the ligaments no longer retained their healthy firmness and strength. This motion gave great suffering. The thigh and leg were placed in a well-fitting case which kept the joint in a state of perfect repose, but allowed the front of its surface to be exposed. Leeches were, from time to time, applied, followed by soothing applications; but she began to lose her rest, and suffered great torture. The startings became very distressing at night; and yet, strange to say, her health was was not visibly impaired; her appetite continued good, and all her functions seemed to be performed normally. Towards the end of June, however, matters began to assume a graver aspect; she suffered from profuse nocturnal sweats; the pain became so intolerable that sleep was impossible; her appetite, for the first time, failed, and henceforth she daily entreated that the limb should be removed.

Such a state of things could not be permitted to continue; and, in consultation with my colleagues, on the 6th July, it was agreed to excise the joint; her age, state of health, and the *apparently* small amount of disease in the ends of the bones leading to the conclusion that it was a most favourable case for operation. Accordingly, on the 15th July, having had her put completely under the influence of chloroform, I proceeded in the following manner:—She was placed on the operating table, in the recumbent position, the limb having been steadied in the most extended position practicable. With a stout scalpel I made a deep incision along the inside of the articulation, four and a half inches in length, commencing about two inches below the head of the tibia, and extending up two and a half above its articulating surface; keeping here in front of the saphena vein. An incision, similar in length, and on the same level, was made on the outside; and in both of these the knife was carried down to the bone. The joint was then fairly opened by a third incision across, about one inch above the insertion of the ligamentum patellæ. At this moment some pus flowed from its cavity. I then dissected the lower flap downwards, and also freed the upper, with the patella lying in it. The lateral ligaments were then quickly divided; the anterior crucial was gone. I now carefully, but rapidly, detached the soft parts from the posterior surface of the femur, when the ends of the bones, on being completely exposed, were found to be almost entirely stripped of cartilage. Having applied a narrow-bladed saw behind, just above the condyles of the femur, I quickly sawed them off. The section of bone appeared of a dark red colour, very soft to the touch; it broke down easily under pressure, and was infiltrated with purulent matter.

The upper extremity of the tibia was found to contain an abscess, which passed downwards to the extent of an inch or more. With this amount of destruction to bone an attempt to save the limb was out of the question, and amputation was imperative.

It struck me that this was a very suitable case in which to adopt Mr. Teale's mode of amputating; and, accordingly, I prolonged the lateral incisions upwards, about five inches, with an amputating knife; and, having cleared the anterior flap from the surface of the femur, I quickly drew the knife across, beneath the thigh, cutting the posterior flap, as nearly as I could guess, one fourth the length of the anterior. Separating the soft parts from the bone, close to the periosteum, a little way upwards, I sawed it through, and, finding the cut surface at this height healthy in aspect, I proceeded to secure the vessels. My next step was to remove the patella from the anterior flap, which was done with facility. I now folded the long flap over the end of the bone, and attached its free angles to those of the shorter one behind by means of two silver wire sutures, the insertion of six more stitches, two in each lateral and two in the transverse line, bringing the parts into good apposition. When the flaps were brought together the anterior seemed too long, so much so that I was almost tempted to shorten it a little, but desisted from remembering Mr. Teale's statement, that "it often appears superabundant, but in result it is not found to be so, chiefly in consequence of the great retraction of the short flap." The patient having been removed to bed, the stump was laid on a pillow covered with waterproof sheeting, and a large piece of wet lint placed over it. With the exception of changing the lint every morning, no other dressing was had recourse to for six or seven days, and any discharge was with ease cleansed by a sponge from the sheeting beneath. The wound healed most kindly, and the ligatures came away at the usual period in amputations of the thigh. It would be uninteresting to describe the progress of this case from day to day; but it will suffice it to add, that the stump turned out in every respect such as was desired. A thick soft mass, devoid of large nerves, covered the end of the bone, was movable on it, and formed a good cushion to bear future pressure. The lithograph, by Forster and Co., from a drawing by Mr. Tomsohn (Plate I.), gives a very accurate representation of the stump when healed.

The chief features of interest in this case, I conceive, are:— The extensive disease in the joint, unaccompanied with proportionate constitutional disturbance until a very late period—the absence of sinuous openings about the joint—the great difficulty of forming an opinion as to the extent of damage to the bones—and the facility with which one operation was converted into another. As far as I am aware, it was the first recorded occasion on which this mode of amputating was performed in Dublin; and also, if I am not mistaken, the first case of its adoption where resection was found not to be practicable, and the immediate loss of the limb required. Since this operation, my colleague, Mr. Collis, performed amputation in the same manner in two other cases. They turned out most successfully, and seemed in every way likely to possess all the advantages said to belong to rectangular flap amputations by the distinguished surgeon who first recommended them.—*Dublin Quarterly Journal, Nov.* 1863, *p.* 262.

36.—NELATON'S PROBE: WITH CASE.

By Dr. F. H. Hamilton, Surgeon U.S.A.

This excellent little instrument has not been brought into the general use and appreciation to which it is already entitled; indeed, its value was fully declared when on its first trial; and in the person of the illustrious Garibaldi, a problem was solved, which had puzzled the most skilful European surgeons. The records of this hospital show several cases where Nélaton's probe has discovered the presence of lead, and, in one case, of iron, where their existence was never suspected, certainly not known. Thus a grapeshot, weighing two ounces, was discovered, and removed by Dr. James B. Cutter, from the calf of the leg; the patient previously averring that the ball had gone "clean through." In this case the iron, being rusted, left upon the probe a brownish stain. In the following case the instrument discovered the presence of lead.

M. G., private 7th Connecticut Vols., was admitted with a gunshot wound of the left leg, a short distance above the ankle-joint. His medical attendants upon the field had informed him that the ball had merely glanced across, but had not entered the bone. The patient was firm in his belief that there was no ball in the wound. Three months after the receipt of the injury, when under my care, at the McDougal General Hospital, the wound showed no signs of healing; and this fact, together with the thin, dark-coloured discharge, led me to suspect the presence of some foreign body, other than dead bone, notwithstanding the statements of the patient to the contrary. I therefore introduced one of Nélaton's probes, and carefully explored the cavity. Removing the probe, and washing off the blood, I found the presence of lead indicated by several marks on the porcelain. Soon after I enlarged the wound, and, cutting down into the tibia on its outer surface, found a small opening into the bone, where the ball had entered. With some difficulty, I then extracted from the substance of the bone, where it was firmly impacted, an entire Minié rifle ball, slightly flattened at its apex. I also removed some pieces of dead bone.

After this the wound proceeded to heal kindly, and was soon entirely closed. Necrosis had already occurred in consequence of the presence of the foreign matter, but by its removal the further extension of the necrosis was prevented. These cases would scarcely, in themselves, be of sufficient importance to merit a special record, were it not that they, as well as any others, serve to illustrate the great value of the instrument. The negative evidence furnished by the probe is often as valuable as the positive, for if, upon a thorough and skilful examination of the wound, we fail in obtaining the evidence of lead or iron upon the probe, we are far more confident in asserting that there is no such foreign substance remaining, than we should be after such an examination with the common gunshot probe.

After withdrawing the probe, the blood, pus, or other matter should be removed, by carefully passing the porcelain ball through a basin of clean water three or four times; when the marks, if there be any, of lead or rusted iron will be left plainly defined upon the surface of the bulb. Of course, these marks should be carefully removed before using the probe again.

It would be well, I think, for each surgeon in the United States service to be furnished with at least two different sizes, one of which ought not to be more than one-eighth of an inch in diameter, and which might be used in searching for buckshot, or in traversing channels made narrow by swelling of the tissues; and the other ought to be about one-quarter of an inch in diameter. This little size, I am satisfied, is large enough for any purpose for which it may be used. If intended for a pocket case, the handle or shaft of the instrument may be made in two pieces, to be screwed together, by which means its length may be increased; or it may be made to fit on the gunshot probe. If the surgeon prefers, also, the two porcelain balls of different sizes may be fastened upon the opposite ends of the same probe, or upon two probes which may be screwed together, by which the length of the instrument could be doubled at pleasure. When the instrument is kept in a pocket case it ought always to be protected by a neat, securely fitting leathern or gutta percha case, such as I have seen made by that ingenious instrument maker, Mr. Tiemann, of New York. In my opinion this probe ought to be added to the instruments which the government supplies as an outfit to the army surgeons.—*Amer. Med. Times.—Dublin Med. Press, Jan.* 20, 1864, *p.* 65.

ORGANS OF CIRCULATION.

37.—ON ACUPRESSURE.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[In the first part of this paper Dr. Simpson considers the impediments to the primary union of wounds, one of the chief of which he thinks is the use of ligatures to the vessels. It is on account of there being no necessity for their employment in what are known as the plastic operations of surgery that we have, as a rule, union by the first intention. Thus the raw edges of a vesico-vaginal fistula almost always cohere, despite the constant contact of urine; the lips of a ruptured and revivified perineum, despite the irritation of the intestinal contents; and the wound in harelip, despite the presence of the saliva. Dr. Simpson then proceeds to ask the question.]

Why do the Ligatures interfere with the Primary Adhesion of Wounds?

Arterial ligatures prevent the primary union of the lips of wounds in two ways—

1st. Each ligature acts itself as a mechanical foreign body placed between the opposed sides of the wound; and when it is composed of silk or other organic material, it rapidly imbibes animal fluids into its substance, which dead fluids speedily decompose and render the threads morbidly poisonous and irritant agents to the contiguous lining tissues. In other words, each arterial ligature is formed speedily into a small or miniature seton; and it produces, like a seton, suppuration along its tract. Suppuration, however, and all the higher grades of inflammation, are locally antagonistic of adhesive inflammation or primary union; and they tend to impede and subvert those processes of cohesion and repair which constitute union by the first intention. But—

2ndly. Ligatures counteract the simple adhesion and healing of wounds in another and far more marked mode. They inevitably set up at all the ligatured points and ends of the tied arteries themselves, stages of inflammation higher than the adhesive—the stages, namely, of ulceration, suppuration, and mortification. For at the point of deligation every ligatured artery has its two inner coats mechanically torn through by the thread, and its outer coat strangulated by it; and before the separation of the ligature can be effected, it requires to cut through the strangulated tube by a process of ulceration or molecular disintegration and gangrene. Further, there are set up, as a consequence, in the immediate vicinity, the processes of local disjunctive inflammation and suppuration; and the part of the arterial tube embraced in the noose of the ligature constitutes a small, dead, decomposing *slough*, as well as the isolated and strangled end of the artery below the ligature, except in instances in which this isolated end is preserved from death by adhesion to the contiguous surfaces. If two, four, or six arteries, therefore, are tied in a surgical wound, then, in consequence of this deligation, there exist two, four, or six points of destructive *ulceration* and *sloughing* in the depths and walls of that wound. And, of course, complete primary adhesion of its sides becomes an improbability, or rather an impossibility.

All our best surgical pathologists acknowledge this ulceration and sloughing as the inevitable and invariable result of the ligature of arteries. Generally the dead tissue is thrown off in small slough masses; to a considerable extent, also, it seems to be melted down and discharged in the form of disintegrated ulceration molecules. We tie and strangulate the necks of piles and polypi to make them ulcerate and slough off, just as the tied and strangulated ends of arteries do. Mr. John Bell, when treating of the effects of the ligature of the artery, uses this very expression:—The part of the artery "below the ligature is (says he) destroyed like a polypus, fades and dies; and it is the fading of the lower part thus *mortified* that allows the noose of the ligature to slip off." Or, to quote the words of Dr. Wise (a very careful observer, and a gentleman who has specially attended to this department of pathology), after deligation of an artery—"an *ulceration* of the compressed part takes place about the fifth or sixth day, and the ligature is slowly discharged about the twelfth or fourteenth day, being thrown off by a *gangrenous* process, together with a small portion of the vessel which had been compressed by the ligature." "Every surgeon knows," observes Mr. Spencer Wells, "that the part of the artery beyond the ligature *must be killed* by it, and that a piece of *sloughy* tissue cannot do any good when confined among the living tissues of the body."—(Medical Times and Gazette, May, 1860.) I might, if it were necessary, cite for you Guthrie, Cross, Brodie, Hodgson, Erichsen, Pécot, Velpeau, Nélaton, and others, as all describing the portion of every artery strangled and isolated by the ligature as killed, and requiring to *slough* off. Let me, for brevity's sake, however, content myself with adducing the statements on this point given by the authors of three of the latest and best works on Systematic Surgery, published respectively in America, France, and England:—"That portion of the artery (I quote the words of Professor Gross, of Philadelphia) immediately embraced by the ligature *mortifies*, and comes away in the form of a *slough*. —("System of Surgery," 1862, vol. i., p. 700). "The portion of artery," remarks M. Chassaignac, of Paris, "embraced by the ligature *necessarily mortifies* a little sooner or a little later, according to the degree of constriction put upon it; and it is only in consequence of an eliminative process, similar to that which in gangrene separates dead from living parts, that it is detached from the tissues which surround it."—("Traité des Opérations Chirurgicales," 1861, tom. i., p. 233). "The noose (of the ligature) and its contained *slough* are," remarks Professor Miller, "to all intents and purposes, foreign matter; as such their presence will be resented by the surrounding living textures; and as such they will be extruded by suppuration."—("System of Surgery," 1863, p. 224.)

If any surgeon, desirous, as all ours are, of producing primary union, had the hardihood to propose, as a new mode of treating wounds (such as those produced by amputation), that there should be placed between their flaps a series of four or five small seton-threads, and that each individual seton-thread should, for a week or two, be firmly fixed and anchored down in the depths of the wound to a corresponding number of small masses of strangulated, dead, decomposing, sloughing tissue—would not such a suggestion, I say, be met by the profession with perfect amazement, and denounced as ridiculous and discreditable, or something worse? Yet, in reality, this, as we have just seen, is the principle followed at the present hour by our surgical brethren, when they staunch the hemorrhages which follow their knife, by tying silken ligatures around the drawn-out and isolated ends of the bleeding arteries. Is it a great marvel that primary union so seldom occurs in wounds so managed? Would it not be a still greater marvel if union by the first intention followed oftener under such adverse circumstances?

We cannot hope, I believe, for any great and decided improvement in the treatment of wounds, and we cannot expect primary union to be frequent in them, till we have attained some means of arresting hemorrhage without strangulating and sloughing the ends of the bleeding arteries. For the accomplishment of this object, other simpler and safer means than acupressure may, and probably will, be discovered; but in the mean time, it has been found capable of occluding wounded and cut arteries without producing upon them any physical laceration, such as the ligature produces, without strangulating and sloughing their isolated extremities, and without leaving any foreign bodies in the wound after the vessels are occluded. Acupressure is founded upon the general pathological principle—that, whilst silk and other dead organic ligatures and substances excite irritation, suppuration, &c., when they are buried in the living structures of the body, there is, on the contrary—

A tolerance of Metallic Bodies by the Living. Tissues.—We have ample proof of the truth of this great general law in many kinds of observation, as in the tolerance—sometimes for long years—of bullets, small shot, pins and needles, imbedded in different parts of the body; in the non-irritating character of threads of iron, silver, platinum, &c., when used as surgical sutures; in the employment by all our best surgeons of metallic pins or needles in the union of hare-lip; and in the safe retention, during several days, of a metallic needle passed through a fold of the peritoneum itself, in the operations of Wutzer, Rothmund, and others, for the radical cure of hernia. The use of a needle in the cure of hernia and of hare-lip—where the whole aim and the deliberate object of the surgeon is to secure and establish adhesion and adhesive inflammation—only shows that a needle, when used to close and compress an artery, as in acupressure, is not likely in itself to lead there to any higher stage of inflammation than the adhesive—and consequently will not interfere, when used as a hæmostatic, with the primary union of wounds. At the same time, let me qualify this remark with another:—if a metallic acupressure needle, or a metallic suture thread, be made to press too long and too strongly on any given point of living tissue, that point will ulcerate. But the ulceration, under these circumstances, is not the result of the pressure of the metallic needle or thread, as metal; it is not, in acupression for example, the result of the pressure of the needle as a needle; but it is the result of the *pressure* of the needle simply as pressure, morbidly excessive in its amount. And any similar degree of linear pressure, by any other mode whatever, will produce the same ulcerative appearances.

That even very long needles may be borne with perfect impunity embedded for days in the living body, is attested by numerous experiments that were performed on the human body, some forty years ago, by various surgeons, when acupuncture was more studied and practised than now. "It is a *remarkable* circumstance (observes a writer on this subject) that the acupuncture needles never cause inflammation in their neighbourhood. If they are rudely handled or ruffled by the clothes of the patient, they may produce a little irritation; but, if they are properly secured and protected, they may be left in the body for an *indefinite* length of time without causing any of the effects which usually arise on account of the presence of foreign bodies. In one of M. Cloquet's patients thay were left in the temples for eighteen days; and in cases in which needles have been swallowed, they have remained without causing inflammation for a much longer period. It appears probable, from the facts collected on this subject, that metallic bodies of every kind may remain imbedded in the animal tissues without being productive of injury."—(See the Edinburgh Medical Journal for 1827, p. 197.)

Effects of Acupressure Needles.—Needles, then, are—as metallic bodies—tolerated by the living structures. To occlude a vessel, they require, as experience has amply shown, to be retained for a few hours, or a few days at most; and further, when passed with this view across the mouth or tube of an artery, they merely place the internal surfaces of the vessel in close contact; without isolating the vessel from its attachments; without lacerating its two internal coats; and without strangulating, ulcerating, and mortifying the constricted portion of the tube, all of which injuries and lesions are, as we have found, inevitably produced when the deligation of arteries is adopted. In thus using against hemorrhage acupressure needles instead of ligatures, we attempt to bring all bleeding wounds in general surgery to the condition, as far as possible, of wounds in plastic surgery, where union by the first intention is the expected and general result. We secure the bleeding arteries by unirritating metallic needles, and we withdraw them as soon as we can, so as to leave ultimately *no foreign body whatever* in the wound. Ligatures cannot be withdrawn for an indefinite period, or till, after days or even weeks, they have ulcerated and sloughed through the tied vessel. We can at once remove, on the contrary, our acupressure needles always at will, and whenever we deem that they have produced their occluding effect. But I am forestalling some observations; for, before considering how short or how long a time the needles should be left in, we have another matter than this to consider first, namely, the—

Apparatus required for Acupressure, and the methods of applying it.—The instruments required for the employment of acupressure are of the simplest kind. They are chiefly applied in three modes. In the first method, the only instrument required is a long needle headed with glass or sealing-wax to allow of sufficient pressure upon it for its introduction; in the second method, a short common sewing needle threaded with iron-wire or with silk; and in the third method, the same needle along with a loop of very slender wire thread of four or five inches in length. The three chief methods of applying the needles are as follows:—

1*st Method.*—This was the mode which I generally adopted in the first acupressure operations. It usually consists in passing a long needle twice through the flaps or sides of a wound, so as to cross over and compress the bleeding artery or its tube, just in the same way as in fastening a flower in the lapelle of our coat, we cross over and compress the stalk of it with the pin which fixes it, and with this view pass the pin twice through the lapelle. The only portion of the needle which is left exposed internally on the fresh surface of the wound is the small middle portion it, which bridges over and compresses the arterial tube at its bleeding mouth, or a line or two or more on the cardiac side of it. More or less of both extremities of the needle, viz., its head and point, are exposed externally on the cutaneous surface of the side or flap of the wound. When passing the needle in this method, the surgeon usually places the point of his left fore-finger or of his thumb upon the mouth of the bleeding vessel, and with his right hand he introduces the needle from the cutaneous surface, and passes it right through the whole thickness of the flap till its point projects for a couple of lines or so from the surface of the wound, a little to the right side of the tube of the vessel. Then by forcibly inclining the head of the needle towards his right, he brings the projecting portion of its point *firmly* down upon the site of the vessel, and, after seeing that it thus quite shuts the artery, he makes it re-enter the flap as near as possible to the left side of the vessel, and pushes on the needle through the flesh till its point comes out again at the cutaneous surface. In this mode we use the cutaneous walls and component substance of the flap as a resisting medium, against which we compress and close the arterial tube. But in some wounds a neighbouring bone or other firm unyielding texture forms the best and readiest point of resistance against which to pin and compress the artery by the acupressure needle. In such cases, the end of the finger at the bleeding point is sometimes necessary to assist the needle in duly pressing it down upon or against the open vessel. In both those modifications of acupressure a thick flap or a vessel situated deeply requires a proportionally longer needle; and the amount of pressure upon the artery is easily regulated and increased, when required, by the acuteness of the angle which the needle makes in its passage over the arterial tube. The degree of compression required to shut an artery by acupressure is generally by no means great, especially if care is taken to pass the needle as near as possible to the arterial tube, and without a layer or layers of elastic tissue intervening between them. This needle can be withdrawn at will, at any hour or time, by pulling at the head of it; which, I have said, is placed externally.

There are some objections to using such long needles in acupressure when they can be avoided. They are liable to be passed so as to compress the included tissues too strongly; they compress, however slightly, an unnecessary extent of tissue; and, being partly external, they are liable to prove unwieldy and incommodious in putting on dressings, &c., to the wound, provided we do use such applications to it.

In the method of acupressure which I have described, the long needles are introduced from the cutaneous surface, and their extremities left out *externally*. In the two other methods, where sewing-needles are used, they are introduced on the raw surface of the wound, and are all situated thus altogether *internally*, or between the lips of the wound.

2nd Method.—A common short sewing-needle threaded with a short piece of iron-wire for the purpose of afterwards retracting and removing it, is dipped down into the soft textures a little to one side of the vessel, then raised up and bridged over the artery, and then finally dipped down again, and thrust into the soft tissues on the other side of the vessel. In bridging over the vessel care must be taken to press the end of the needle down upon the mouth or tube of the bleeding artery with force sufficient to arrest the hemorrhage. The end of the finger pushed against the projecting portion of the point's-end of the needle is often required thus to compress and close the vessel adequately, before the tip of it is sent outwards and fixed in the tissues beyond.

3rd Method.—This consists in compressing the vessel between the threaded sewing-needle and a duplicature of passive iron thread. Here, as in the method last related, the cutaneous surface is left intact; but the needle is passed *below*, instead of over or above, the artery. The point of the needle is entered a few lines to one side of the vessel, then passed under or below it, and afterwards pushed on so that the point again emerges a few lines beyond the vessel. The noose or duplicature of wire is next thrown over the point of the needle; and, after being carried across the mouth or site of the vessel and passed around the eye end of the needle, it is drawn sufficiently tight to close the vessel, when it may be fixed by a half-twist or twist around the stem of the needle. A slight half-twist usually fixes a rigid wire-thread sufficiently. If the operator prefers, he may keep the two threads of the noose open after they bridge across the artery, and *tie* them below or behind the eye end of the needle, in the form of a common single or double knot. But this tie takes much longer time than the twist, and is not more efficient. When in either case the operator wishes to remove this simple acupressure apparatus after a period of, say five, twenty, thirty or more hours, all he has to do is to withdraw the needle by pulling it out, by dragging at the twisted wire with which it is threaded. The noose of wire-thread is thus at once loosened and liberated, and can be withdrawn also. To distinguish easily between the wire-thread passed through the eye of the needle and the duplicature of wire, it is always convenient to make the former by having it plaited or twisted.

[When should the needles be withdrawn? Much more investigation and practice are required before this and some other questions regarding acupressure can be answered. Dr. Struthers was the first surgeon who had the boldness to apply acupressure to the femoral in an amputation of the thigh—he did not remove the needle from the artery till the fourth day, or about ninety-eight hours. In a successful amputation of the thigh, also for traumatic spreading gangrene, Dr. Handyside removed the needle over the femoral in forty-nine hours.]

In the following instance of amputation of the thigh for chronic pathological disease, performed by Mr. Crompton, Senior Surgeon to the General Hospital, Birmingham, the needles were left for fifty-two hours :—

Case 1.—*Amputation of the Thigh for Scrofulous Disease.—Needles Removed in Fifty-two Hours.*—On February 29, 1860, Mr. D. Crompton amputated a young man's thigh, for strumous disease in the femur, and consequent degeneration of the cartilages of the knee-joint. Two acupressure needles sufficed to arrest the bleeding, one of them pressing on the femoral artery, and the other pressing upon two small muscular branches. After fifty-two hours, the needles were all withdrawn. On March 29, or exactly four weeks after the operation, Mr. Crompton sent me a cast of the stump, with a note, stating that the young man was then quite well. In this case no rollers or plasters, nor, indeed, dressings of any kind, were applied to the stump.

In the following case, reported to me by my friend and former pupil, Dr. Hamilton, the needle was removed from the femoral artery after forty-eight hours. The case occurred in the practice of a very able young surgeon—Mr. Brown, of Carlisle.

Case 2.—*Thigh Amputation and Primary Union—Needles Removed in Forty-eight Hours.*—The patient was a man of fifty years of age, and had suffered for two years from ulceration of the cartilages of his left knee-joint. Two sinuses connected with the joint were discharging profusely pus of a very offensive character. The poor man was reduced to such a state of emaciation and debility, that when he came down from near Carlisle to Edinburgh for the purpose of undergoing amputation, one of our most distinguished surgeons refused to undertake the responsibility of operating. He, therefore, returned to Carlisle, where amputation was performed by Mr. Brown, after Teale's method, at the lower third of the thigh, and five bleeding vessels were acupressed. Forty-eight hours after the operation all the needles were withdrawn. Primary union took place throughout, except at a single spot where one of the sinuses, already alluded to, had been cut across. The patient was able to be dressed and down-stairs within four weeks, and in six weeks he was driving out alone in his gig daily; his general health good, and his strength rapidly returning.

Local Requisites for the Primary Union of Wounds.— The closure of extensive wounds in which acupressure is used to arrest the attendant hemorrhage, is still the exception, and not the rule. It is one of the elements, and, as I believe, a very important and necessary element, in our attempts to effect primary union; but there are other elements and circumstances which must also be most carefully attended to. The sides or flaps of the wound must, if we expect primary union of them, never be of a kind to require pulling and tension, either to bring them together or to keep them in apposition. If they are too small and short for this purpose, or if the stitches holding them require to be put or kept on the stretch, then union by the first intention becomes scarcely possible. Surgeons will probably come yet to shape their wounds and flaps more systematically than they do even at present, so that the sides and edges of their resulting wounds lie fully and easily in contact. If the sutures, whether metallic or silken, be placed upon the strain, they will cut through the lips of the wound by ulceration to the extent necessary to relieve the strain. When the lips of the wound are ultimately adjusted, and accurately and carefully fitted to each other, no one of the sutures must be found on the stretch and deeply indenting the points on which it rests and presses; for, if so, those points will soon become irritated and ulcerated, and the act of primary union will be more or less interfered with. But still more, when we strive for primary union, we must beware of leaving any foreign and dead materials—however small and almost molecular they may be—on the sides or in the cavity of the wound. In an amputation wound, when the saw divides the bones, the resulting dead particles of bone-dust are freely scattered all around. Any of these particles left on the surface of the wound would infallibly prevent primary adhesion at the points where they were lodged; nay, they would sooner or later set up suppurative inflammation ere they were finally discharged. They must all be carefully removed by sponging, or, still better, by a stream of water allowed to fall upon the wound; for even our sponges sometimes leave flakes and fragments of their tissue upon the surface of the wound, which, though they may be minute, are still dead foreign bodies. Of course, it is needless to close up a wound and expect it to heal by primary union if you allow coagula of blood to remain lodged within it. These coagula act again as foreign bodies, both inevitably preventing present adhesion, and inevitably producing future suppuration. The wound must not be shut up till all bleeding has ceased; and to prevent the mischance of any coagula forming for the next few hours, some surgeons do not close their wounds till six or eight hours have elapsed. In closing large wounds, particularly large amputation wounds, it is further, I am inclined to believe, a matter of great moment to bring the flaps most carefully together from below upwards, so as to preclude the possibility of a small collection or collections of atmospheric air being left in the depths or between the approximated sides of the wound. I fancy that I have seen the flaps brought together first, and chiefly along their edges, so that a quantity or quantities of air were left lodged within. Like any other foreign body which keeps the opposed walls of the wound from approximating and touching perfectly, air, even in small quantities and bubbles, will as effectually prevent primary adhesion at the parts or points at which it is located as the same bulk of dead solid materials would. Whilst it is thus a great and indispensable indication to free the surfaces of the wound from all foreign matters that can possibly prevent them coming everywhere into close and accurate contact, it is sometimes necessary, also, by the position of the wounded part—by the use, if necessary, of slight bandages, and by supporting sponges or light pads placed externally—to retain the opposed surfaces in the same close contact till they are glued together in sufficient strength by the reparative process. In other words, it is necessary, not only to place the sides of wounds in accurate contact, but it is necessary also to *keep* them thus in accurate apposition. The mere shallow cutaneous edges only of large wounds are usually brought together by the superficial stitches that are used; but if the stitches be made by metallic sutures planted so deep as to embrace the sides of the wound to a considerable extent, bandages, plasters, and dressings are very seldom required. The mere atmospheric pressure keeps the sides of a wound in contact when once its lips are accurately closed and sealed up with an adequate number of metallic sutures. And certainly the fewer local dressings and applications we require to place about a wound, the greater, perhaps, is the chance of its successful repair and reunion. I believe, in other words, that, after the sides and edges of a wound are properly approximated and adjusted with its metallic stitches, the best dressing, as a general rule, is—nothing, absolutely nothing. I have seen full and testing proof of this in the large wounds left by excision of the mamma, and by ovariotomy.

We have not easily the power of applying any dressings, or ointments, or lotions to cases of vesico-vaginal fistulæ immediately after they are operated upon; and it is, perhaps, fortunate that it is so; for, probably, these vesico-vaginal wounds close by primary union with the comparative certainty, and in the large proportion which we see in practice, greatly because we are shut out from intermeddling with them, therapeutically or surgically, after they are once stitched up. We cannot overload and overheat them, as was too often done in former times, in "dressing" external wounds with complex layers of ointments, pledgets, straps, rollers, &c. Even a dressing of charpie and cold water to a recent wound is, perhaps, more hurtful than useful. It busily unmakes what nature is busily making—a crust along the edges of the closed wound. If the lips of it become red and irritated or inflamed, apply to them cold air, and you will find, as I have found, that a stream of it directed upon the wound or its vicinity from a pair of bellows will prove both beneficial locally, and most grateful to the feelings of the patient.

[In answer to the question, "*Is not the Acupressure more difficult than the Deligation of Arteries?*" Dr. Simpson observes :]

In its present condition, however, the surgeons who have tried it have found it as easy, at least, as the ligature. After using acupressure in two cases of amputation of the fore-arm, Dr. Greig, of Dundee, wrote me that he found the needles "applied quite as easily as the ligature." "The process (says he) so far as I have tried it, is *the simplest* that one can imagine. . . . . It is really surprising how very little pressure is required to stop bleeding from an artery." "In giving directions for securing the vessel, you advise the surgeon to place the forefinger over its bleeding mouth, &c. Now, you will find it much better, when you have a flap, to keep the forefinger of the left hand on the skin side, and use the thumb. You feel the vessel beating between the thumb and the forefinger, and you *can introduce the needle in the dark.*"

In the first case of amputation in which Dr. Handyside used acupressure, he had to undertake the operation at a distance in the country, and improvised some long "darning" needles into acupressure implements for the occasion. He fixed knobs of red sealing-wax upon the heads of them to facilitate their introduction. At that time the two easier modes of effecting acupressure by common short sewing needles had not yet been thought of. But, in order to show you that Dr. Handyside found no difficulties in the application of acupressure even with the long needles, let me state in a few words his case, which was a very interesting one, and then quote the comments which he has himself published on it in reference to the question of the relative facility or difficulty of acupressure.

Case 7.—Amputation of the Upper Third of the Thigh for Traumatic Spreading Gangrene..—The patient received a very severe lacerated wound of the leg, with compound fracture of both bones, from a loaded waggon running against him down a steep incline. Gangrene supervened, and in five days afterwards, when Dr. Handyside was summoned from town to see him, he was delirious and prostrated; the mortification had reached the knee, and was spreading upwards; the inguinal glands were much enlarged, and the lymphatics of the limb tense; the pulse rapid, weak, and irregular; and the skin cold and clammy. Dr. Handyside performed amputation of the thigh immediately below the trochanters, and arrested the bleeding from four divided vessels by as many needles. Two of the vessels were relieved from acupressure at the twenty-fourth hour, and the other two—one of them being the femoral artery at its giving off of the profunda branch—at the forty-ninth hour. The swollen glands in the groin suppurated, it would seem, and the pus was partly discharged from the centre of the cicatrix, which formed at the line of union of the flaps, and partly from two incisions made through the recent adhesions by Dr. Todd. Notwithstanding all the unfavourable circumstances of this case, "the line of cicatrisation of the flaps," to use the words of Dr. Todd, "was healed by first intention," and in twenty-one days the patient was in excellent health, and had a cast of his stump taken in plaster.

[The amount of force used in tying an artery is not a criterion of the amount required to arrest hemorrhage from its cut orifice; it only shows the amount required to cut through the two internal coats of the vessel. Properly used acupressure is as effectual in arresting hemorrhage as is the application of a ligature.]

But will acupressure prevent *secondary* hemorrhage as effectually as deligation?

The common local causes of secondary hemorrhage in deligation consist of ulceration or sloughing of the arterial walls—either too rapid in point of time, or too great in point of degree—by which the cavity of the artery is opened before the internal plug of blood and fibrine, formed above the ligature, has completely occluded its canal. Mr. John Bell tells us that "secondary hemorrhage arises from ulceration of the artery more frequently than from any other cause." ("Principles of Surgery," vol. i., p. 321.) Secondary hemorrhage "may occur (observes Dr. Druitt) from *sloughing* or from *ulceration* of an artery; or from imperfect closure of an artery when a ligature separates." Or, according to Mr. Syme ("Principles of Surgery," page 89), "if a ligature has been applied, the ulceration by which it is separated, if too rapid or extensive, may cause a bleeding from three days to as many weeks after the infliction of the injury."

In every case of deligation there is more or less ulceration and sloughing as an unavoidable consequence of the use of the ligature. It is when one or other, or both, of these processes go to a morbid excess and depth, that secondary hemorrhage from the opened arterial tube follows. But, in acupressure, there is no required ulceration or sloughing at all of the arterial tube; its cavity is obliterated merely by laying its internal surfaces in contact by the pressure of the needle. And hence, secondary hemorrhage as the result of its two common causes—viz., ulceration and sloughing—should be almost or altogether unknown when acupressure is resorted to.

But in the present limited state of our knowledge regarding the mode or mechanism of the occlusion of arteries by acupressure, and the time required for it, acupressure has been followed by secondary hemorrhage in two amputations out of the pretty long list in which the vessels in stumps have been acupressed. These cases and their causes are, therefore, considerations of no small moment. In the first of the two, probably the needle was withdrawn too early, and before the sealing up of the arterial tube was fully accomplished. In the second case, the process of internal closure of the tube by an internal plug and cohesion was probably delayed much beyond the usual time by the state of impaired health and impaired blood of the patient. The first has been reported by Dr. Hamilton in his Carlisle list of cases:

Case 8.—*Amputation of the Thigh, with Acupressure and Secondary Hemorrhage.*—A boy, aged five and a half years, underwent primary amputation of the thigh, at its lower third, for a railway accident, which had mangled the leg. Twenty-four hours after the amputation, Mr. Page withdrew the only two acupressure needles that had been required, when bleeding immediately occurred to the extent of two ounces. The dressings were at once removed and the stump raised, when the hemorrhage ceased. There was no tension or redness of the flaps. The blood escaped by the opening left by the wires of the acupressure needles. Except at this point, there was union by the first intention along the whole line of incision. For the next two days, the pulse, as on the first day, was high, with vomiting and much tossing. After a fit of restlessness, hemorrhage recurred from the wound forty-one hours after the withdrawal of the needles. It ceased under manual compression of the femoral and raising of the stump. "Having," says Dr. Hamilton, "adjusted a tourniquet over the vessel high up, I forcibly tore asunder the flaps, which were adherent from end to end, except at the small orifice by which the blood escaped on the 26th: then clearing out some newly-formed clots, exposed the bleeding orifice, which proved to be that of the popliteal. I then passed a needle under it, and secured it in the usual manner. The hemorrhage being completely checked, the flaps were approximated by four points of wire suture. On this occasion it was calculated that between four and five ounces of blood were lost." There was no recurrence of hemorrhage. Sixty-nine hours after the introduction of the needle, it was withdrawn; and within five weeks after the amputation, the wound from it was quite closed and healed.

Perhaps, in this instance, the needles were withdrawn too early—that is, within twenty-four hours after amputation of the thigh. More evidence, however, as I have already argued, and more experience are required on this point before any very fixed principles can be laid down. I will betimes have occasion to speak of a case of thigh-amputation, where the needles were withdrawn in a young child within twenty-four hours, and without bleeding following. In conversation upon the preceding case, Dr. Hamilton informed me that he was inclined greatly to attribute the second attack of hemorrhage to the little patient's great restlessness.

Perhaps I may also be allowed to add here, that in Dr. Hamilton's report of amputations at the Carlisle Hospital, there are given eleven cases where the ligature was used, and eleven were acupressure was employed. Among the latter eleven there was only the preceding case of secondary hemorrhage. Among the former eleven, where the ligature was used, there were four cases of secondary hemorrhage, and two of these were cases of fatal secondary hemorrhage. In other words, there happened in that institution four times more cases of secondary hemorrhage from the ligature than from acupressure.

Case 9.—Amputation of the Thigh—Secondary Hemorrhage after Acupressure.—In the case of a phthisical young man, of twenty years of age, Mr. Crompton performed circular amputation in the lower third of the thigh, on account of severe compound comminuted fracture of both tibia and fibula,—the result of an engine-tender having passed over his leg. The femoral artery was secured by an acupressure needle, and two small muscular arteries were twisted. After seventy hours the stump was in great measure united, but there was still pulsation over the needle near its point of exit. Mr. Crompton, therefore, hesitated about withdrawing the needle, but, as there was considerable inflammation and suppuration in its track, he ventured to remove it. No hemorrhage took place at the time, but two days afterwards Mr. Crompton found four or five ounces of coagulated arterial blood on the bed, and more blood still oozing from the depending part of the incision. He at once reintroduced the acupressure needle a little higher up, and thus arrested the bleeding completely. The hemorrhage was attributed to the patient having made use of the bed-pan about half-an-hour before. That same day slight symptoms of phlebitis had been noticed. Shivering and vomiting soon followed, and the patient became delirious at times, and died of pyæmia on the tenth day after the operation. On examining the body, "the external iliac," to use Mr. Crompton's words, "and the common and superficial femoral arteries on the right side, were found quite empty, even down to the position of the needle, and the lining coat was of its natural colour, except at the point of pressure, where there was a line of blue discoloration. The external iliac and common and superficial femoral veins were filled with decomposed clots and sero-purulent fluid. This state did not extend into the common iliac vein or the vena cava." The apex of the left lung was "adherent," says Mr. Crompton, "containing several small cavities. Around them, for two or three inches, the tissue of the lung was filled with tubercular matter."

In this patient, it would appear that the internal consolidation and organisation of blood or fibrine necessary for the permanent occlusion of the injured artery had never properly taken place. Whether this deficient action was owing to the previous low state of the young man's health, as testified by the tubercular disease in his lungs, or to the depression resulting from the injury and the operation, or to some other cause, it is not, perhaps, possible to determine. It possibly would have occurred equally with the ligature. But an important lesson in acupressure practice is surely read to us by this interesting case; for, according to the very sagacious observation of Mr. Crompton, the pulsation could be felt going on along the artery down to the very point of compression by the needle; and the lesson, I take it, is this—that so long as the arterial pulsation in any case may be felt close to the needle, as can always be done immediately after its insertion, we may regard it in any doubtful case as an indication that perfect consolidation and permanent occlusion have not yet taken place, and that the time for the withdrawal of the needle has not yet arrived.—*Medical Times and Gazette, Jan.* 2, 9, *and* 16, 1864, *pp.* 1, 25, 53.

38.—REMOVAL BY LIGATURE OF LARGE SUBCUTANEOUS NÆVI WITHOUT LOSS OF SKIN.

By J. Jardine Murray, Esq., Honorary Surgeon to the Brighton and Hove Dispensary, and Surgeon to the Brighton and Sussex Eye Infirmary.

Nævi may be divided into three classes—the cutaneous, the subcutaneous, and the mixed; and as each of these requires different treatment, a correct diagnosis is of essential importance. Moreover, the nævi of each class vary so much in structure, form, and position, that no one method of treatment is applicable to all; and, in fact, it is often requisite to use various modes of treatment in the same case.

Although it is probable that the majority of nævi would disappear spontaneously, there is perhaps no malformation or disease which is more frequently subjected to operations in children. But in general the friends are too impatient and the surgeon is too zealous to afford nature a chance. In hospital and dispensary practice I have had many of these tumours under my care, and have attentively watched several cases in which they have disappeared without active interference, as the children grew older. In the subcutaneous or areolar nævus this spontaneous disappearance seems to be less common than in the other varieties; but during the last two years I have been watching with much interest a pure case of subcutaneous nævus of the scalp, in which nature is slowly and painlessly effecting a cure. Doubtless the tumour is undergoing degeneration and absorption; for, from month to month, there is less and less of the soft spongy mass to be felt, and the skin covering the tumour becomes looser. Of late I have abstained from operations except in those instances in which the nævi continued to increase steadily, or where the impatience of the relatives gave me no alternative.

It is not my present intention to criticise the very numerous expedients which have been suggested for the removal or cure of nævi; but I wish shortly to refer to a plan by which such of these tumours as are entirely subcutaneous may with ease and certainty be cured by ligature, without destruction of skin or production of an unsightly cicatrix. I shall therefore describe a case of this kind, together with the mode of cure; and then state what appear to me to be the advantages of the method of operating which I was led to adopt.

Elizabeth S., a fine child, aged two years, was brought to me in November, 1862. On examination, I found, about an inch inch and a half above the umbilicus and a little to the left of the linea alba, a large globular swelling with its long diameter directed upwards. The skin covering the tumour was natural in appearance. When grasped between the fingers and thumb the substance of the swelling felt soft, doughy, and elastic. It appeared that by firm pressure the tumour might be reduced

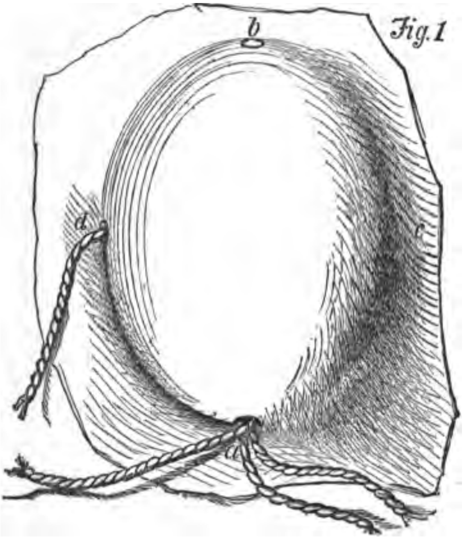


Fig. 1 represents a nævus of the subcutaneous variety. The object is to remove the abnormal structure by ligature, without destroying the healthy skin. In this figure all the steps of the operation are supposed to have been performed, except the tightening of the ligature.

in size; but when the child cried the swelling became tense and hard, and perhaps also a little darker in colour. Evidently the case was a well-marked example of subcutaneous nævus. I directed that a truss which had been worn, under the mistaken notion that the swelling was an umbilical rupture, should be laid aside. I watched the case during the next three months, and became convinced that the mother had good grounds for her belief that the tumour was rapidly increasing in size; and I therefore yielded to the urgent request of the parents that I should operate for its removal. The size and appearance of the nævus at the time of operation are represented in Fig. 1.

On the 19th February, 1863, Mr. H. Philpott administered chloroform to the child, and, with the valuable assistance of Mr. H. Blaker, I operated in the following manner:—

The Operation.—The needle (Fig. 3), armed with a double thread of very strong silk, transfixes the base of the tumour from *a* to *b*. The silk emerges at *b*, and is thence drawn out and cut close to the needle. The left hand thread is then slipped through the eye of the probe (Fig. 4), and carried round the tumour close under the skin from *b* to *d*, where it again emerges. It is now attached to the needle, and passed across the base of the tumour from *d* to *c*. Thence it is again conducted under the skin by the probe, and finally brought out at *a*. It is evident that 2-4ths of the tumour are enclosed by this thread, which, for the sake of clearness, is represented in the diagram (Fig. 2) by the thin dotted-line.

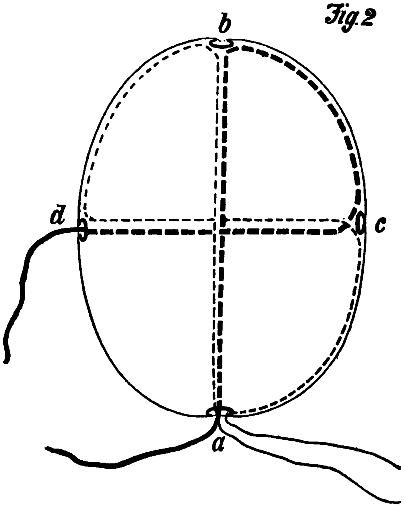


Fig. 2.—Diagram to show the mode in which the ligatures include the tumour.

The second thread (represented by the dark-dotted line in Fig. 2.) must now be attached to the probe, and carried under the skin to *c*, and then by the needle through the base of the tumour to *d*. The noose so formed includes the remainder of the tumour, and the thread may be tightened over an incision through the skin from *d* to *a*, made for the purpose of providing an exit for the slough of the nævus; or both ends of the second thread may, like the first, be brought out and tied at the point *a*. I am now disposed to think that the incision from *a* to *d* may be dispensed with, for there does not seem to be much difficulty in the separation of the slough, of which the chief portion finds a ready egress with the steady discharge of pus from the apertures in the skin. Whatever of indurated swelling remains, after the separation of the ligatures and the healing of the wounds, is gradually removed by absorption.

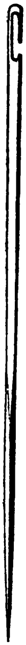


Fig. 3

Fig. 3.—Needle for carrying the silk through the tumour by transfixing its base.

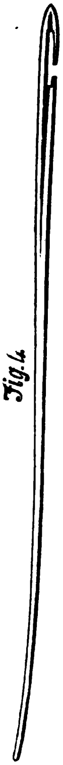


Fig. 4

Fig. 4.—Silver probe for carrying the silk round the circumference of the tumour, close under the skin. To facilitate the threading and unthreading of the needle and probe, each is made with the eye open at the side.

There was hardly any bleeding, as the silk filled the punctures made by the needle. On tightening the ligatures, the tumour was rendered more prominent and very hard, and the skin was slightly puckered. As on the third day the child was feverish and restless, and the skin red and swollen, poultices were applied. A healthy discharge of pus was soon established. On the tenth day, in order to hasten the separation of the ligatures, which had become loose, a piece of bougie was passed through the loop of each, and the portions of the base of the tumour encircled by the silk were from day to day constricted by torsion. On the fifteenth day the whole of the base of the tumour had been cut through, and both ligatures came away. After this the discharge gradually diminished and the wound healed satisfactorily. The child did not suffer in health or appearance. On examining the little patient on December 21, 1863, ten months afterwards, the tumour was found to be gone, and no trace of the operation could be seen, except a very faint white cicatrix marking the line of incision (*a d*, Fig. 1). It is evident that subcutaneous nævi on the face or any other conspicuous part of the body may be cured in the same manner; and as there need not be any destruction of skin, there will be no enlarging cicatrix,—for it is too often forgotten that cicatrices grow.

When I operated as above described, I was not aware that a like method had been previously adopted by any one, but some months afterwards Mr. Birkett referred me to the record of a case of subcutaneous nævus which he had endeavoured to cure after a somewhat similar manner. If, when one fancies he has hit on some improvement in medical or surgical practice, he but take the trouble to search the records of our art, he will probably find that his ideas have been anticipated; and indeed it would be surprising if such were not generally the case. Accordingly I find that Professor Pitha, Mr. Curling, and Mr. Adams have each tried a plan resembling that which I have described, but apparently with very uncertain results. I believe that the want of success which attended most of their operations may be attributed to the circumstance that the ligatures were conducted round the nævus under the skin by a sharp-pointed needle. Now it seems evident that in the attempt to encircle the subcutaneous structure in this manner a portion of the vascular tissue is more likely to escape the ligature than would be the case if a probe were used. For the sharp-point of the needle cannot be guided close under the skin without risk of piercing it; whereas the point of the probe can be pressed firmly along the under surface of the skin, so that none of the vascular structure of the nævus is likely to escape being included in the noose of the ligature. And it is clear that any portion of the vascular tissue may suffice to carry on such an amount of circulation in the tumour as may result in the re-establishment of the disease.

It also appears to me that when the growth is of large size its base can be more thoroughly strangulated by dividing it into four portions, a plan which does not seem to have been hitherto attempted in the treatment of subcutaneous nævi.

Finally, I may remark that, although the operation which I venture to recommend seems complicated on paper, it is simple in practice. As the needle and probe have each the eye open at the side, the threading and unthreading is accomplished very quickly and easily.—*Lancet, March* 19, 1864, *p.* 321.

39.—ON A NEW MODE OF ARRESTING VENOUS HEMORRHAGE AFTER AMPUTATION.

By George H. Porter, Esq., Examiner in Surgery, Royal College of Surgeons, Ireland, &c.

In amputations, when the circulation has been controlled by pressure with the finger of an assistant, or by some of the more modern tourniquets, such as Signoroni's, Skey's, or that of Salt, which compress chiefly the main vessels, it is not by any means uncommon for the operator to be annoyed by venous hemorrhage. This can be explained by the fact that, the smaller arterial branches arising above the site of the pressure, carry a considerable quantity of blood to the limb below the point compressed;

and the blood is prone to pass from the veins accompanying those arteries into the lower part of the larger ones, and flow from their divided mouths. Generally speaking this subsides on placing the stump in an elevated position, or by pouring a stream of cold water over it; but, on the other hand, I have frequently seen a surgeon, after all the arteries had been carefully deligated, obliged to keep the cut surface exposed to the air for a long time, with the point of his finger fixed on a vein, before the flow of blood could be checked. Again, I have witnessed such bleeding arrested only by placing a pledget of lint with a string attached, for its removal, on the vessel's mouth. This, independently of retarding, to some extent, the healing process, caused a large amount of suffering on its withdrawal some hours subsequent to the operation; and may also be objected to on the ground that it keeps a supply of pus, putrid blood, or serosity, in constant contact with the orifice, for absorption. Surgeons in this country are not favourable to the practice of ligaturing a vein, although having the sanction of such authorities as Hey, Desault, and Hennen. The dangerous, and sometimes fatal, phlebitis following such procedure, has given us a wholesome dread of tying, or, in any way inflicting injury on these vessels. Again, I should much fear that placing a ligature round both vein and artery—as, for example, the femoral vein and artery—(Desault) would prevent the cord having the desired effect on the latter, as the vein must hinder it from dividing the internal and middle coats of part of the tube, and thus render the possibility of secondary hemorrhage to be apprehended. It occurred to me that the trouble might be got rid of by the simple contrivance of temporarily grasping the mouth of the vein within the jaws of a very small Dieffenbach's artery forceps, having connected with it a string to take it away when all bleeding had ceased. Its pressure effectually controls the flow of blood; its bulk forms no obstacle to bringing the parts into close approximation; and it can be removed with the greatest ease, almost, indeed, with the same facility that we draw away a ligature. At my request my colleague, Mr. Collis, lately tested the plan with the most marked advantages, in a case of amputation of the thigh, in which bleeding from the femoral vein proved troublesome. My most sanguine expectations were realized by its action, as it instantly sealed the vessel, and so guarded against further hemorrhage and purulent absorption. It inflicted no injury; and, in the case above-mentioned, it was pulled away in forty-eight hours without causing any pain to the patient.—*Dublin Quarterly Journal, Nov.* 1863, *p.* 268.

40.—ON VARICOSE VEINS.

By Frederic C. Skey, Esq., F.R.S., President of the Royal College of Surgeons, and Surgeon to St. Bartholomew's Hospital.

Of cases of varicose veins of the leg every hospital furnishes abundant examples. Whether or not coupled with what is loosely denominated a varicose ulcer, they are often a source of much evil to the subject of them. They are the cause of both weakness and pain. They incapacitate for hard work. But they do not exist alone. Their presence marks a constitution; it is the constitution of debility—a deficiency of power in the acting organs of the circulation. The treatment involves two objects: 1st, the increase of power in these organs; and 2nd, the turning the current of the venous circulation into healthier channels. The first is effected by the liberal administration of nutrio stimulants. The second object has tested the inventive faculties of many surgeons. I leave it to others to commend the various schemes adopted by them. I discountenance, from long observation of its incompetency to cure, the employment of the needle, whether through the vein or under it, single or double. It has these objections: 1st, it is not unattended with danger; and 2nd, it fails to obliterate the vein, except at the point of its application, mainly because the applications cannot be safely made in numbers proportionate to that of the veins affected. I have at present in St. Bartholomew's Hospital a woman under treatment for varicose veins of the leg, whose limb was jeopardized by the employment of the needle a year ago. A long illness, with severe inflammation and extensive abscesses, followed. The same limb is again under treatment for the original disease. There is no danger in making any number of small eschars on the most projecting surfaces of varicose veins, if made with an escharotic composed of two-fifths of pure potash and three-fifths of powdered lime. This powder, well combined, is made into a paste with alcohol. Whether other escharotics are dangerous in their operation on veins I do not stop to inquire; I only know that the Vienna paste, combined as I have above described it, is not. These eschars may be made in any number proportionate to the extent of the disease. I have treated perhaps 250 cases in the course of the last ten years, and I continue to treat them, by the same means. The paste is applied over the most projecting parts of the vein in the following manner: through a series of about four layers of adhesive plaster a circle is cut of the size of a threepenny-piece or smaller. The influence of the escharotic extends through the vein; and it is curious to observe that from the hour of its application the entire vein appears to be obliterated, and is undetectable to the finger on pressure. From ten to twenty-five eschars may be applied between the ankle and the knee. Twenty minutes suffice for the full operation of the escharotic, and an average of one month for the cure. In very weak constitutions the ulcers will heal very slowly, unless well-directed efforts be made to give force to the general system.—*Lancet, Jan.* 2, 1864, *p.* 1.

41.—ON THE APPLICATION OF INDICES TO ANEURISMAL CLAMPS AND OTHER PRESSURE INSTRUMENTS.

By Ernest Hart, Esq., Ophthalmic Surgeon to St. Mary's Hospital.

The marked want of success which had attended the use of clamps and tourniquets in this country, as applied for the cure of aneurism, especially popliteal, by indirect pressure on the femoral, had, the author observed, become very evident to him in collecting published cases, and comparing the results in English hospitals with those which had been obtained by the Irish surgeons, who felt for this method of treatment all the interest which parentage could give. It was unnecessary to give figures or details for the purpose which he had in view; and he need only say that the analysis of the unsuccessful cases showed that the greater part had failed through the pain caused by the pressure, the ulceration or sloughing of the skin under the pad, or the extreme tediousness of the process, causing its abandonment as ineffectual, and recourse to the ligature of the main artery. Ligature after compression had failed gave less satisfactory results than primary ligature. In thirty-seven cases treated by English surgeons, in which it had been employed after compression had failed, there were fourteen deaths.

The sloughing, ulceration, and excessive pain, in a large number of the cases which he had mentioned, seemed to him to be the consequences of defective application of the pressure. To maintain continued and graduated pressure upon the femoral artery, even with any of the best compressors, without exerting too much on the one hand, and yet controlling the flow of blood on the other, was by no means easy, and required skilled and constant attention; and a certain amount of loving care was needed to prevent excess or defect. The most usual faults on the part of those entrusted by the hospital surgeon with watching the case were, employing an excess of pressure, or exerting pressure in a wrong direction; so that power was wasted in compressing muscles, and the artery was not fixed against the bone. The surgeon placed the pad carefully in position, but it must presently be relaxed, and then the assistants commonly erred in greater or less degree in re-applying the pressure.

The application of indices to the instruments was likely, the author thought, to obviate much of this difficulty, and to remove some prevalent causes of failure in instrumental compression. The surgeon, on putting the pads in position, could in each case ascertain for himself with what amount of pressure the femoral circulation could be stopped, or slackened, according as he might desire to produce the one or the other effect. Having determined the minimum force by which this could be effected, he could then inform his assistants, and point out to them the limits within which they must keep the pressure. If, in replacing the instrument subsequently, they found that the circulation was not controlled with that degree of pressure, they would know that the pad was not well placed; and instead of screwing-down the clamp, and increasing the pressure until the pulsation was stopped, they would be careful to improve the direction of the pressure. It would be very useful for the surgeon himself to know what amount of pressure he was employing to stop the flow of blood; and it would be still more useful for the assistants to have under their eyes a constant monitor as to the correctness and care with which they were carrying out the surgeon's directions.

To give a long table of estimations of the degree of pressure in different subjects would not, the author thought, serve any useful purpose, as this would need to be ascertained for each particular case at the commencement of the treatment. He might, however, just select the three following from his notes of the results of experiment on this point, as giving average figures for three typical subjects:—

A. D., adult male, 5 ft. 7 in. high, somewhat emaciated after illness; measuring (at the level of the perineum) round the thigh 15 in., above the knee 12 in. Pulsation arrested at apex of Scarpa's triangle by a pressure of 7 ℔s.; at Hunter's canal by a pressure of 8 ℔s.

J. R., adult male, 5 ft. 6 in. high, robust and powerful; measuring (at the level of the perineum) round the thigh 22 in., above the knee 14½ in. Pulsation arrested at apex of Scarpa's triangle by a pressure of 11 ℔s.; at Hunter's canal by a pressure of 14 ℔s.

A. N., adult male, 5 ft. 4 in. high, of slight and feminine build; measuring round the thigh at the level of the perineum 20 in , above the knee 12½ in. Pulsation of femoral arrested at fold of groin by a pressure of 4 ℔s.; at apex of Scarpa's triangle, by 10 ℔s.; at Hunter's canal, by 11 ℔s.

In the instrument made under the author's direction, by Messrs. Whicker and Blaise, and which he offered for inspection, the pressure is registered by a needle on a scale, and it is effected by a strong spring, which affords a pressure capable of nice graduation from four to twenty pounds. This instrument had already been tested in practice, and had worked well. Mr. Hart owed to the kindness of the Director-General of the Army Medical Department the report of a case in which it answered expectation in curing a popliteal aneurism. The analysis of cases showed that elastic pressure was best borne.

The principle involved in the application of indices seemed to the author not unworthy the attention of the society, for it was obviously one which might be applied with advantage to other instruments of pressure and extension in surgery; and on the table would be seen an index of a similar kind applied to pulleys for reducing dislocations. It might be useful to apply them to apparatus for overcoming fibrous anchylosis, and restoring bent and deformed limbs.—*Lancet, Dec.* 19, 1864, *p.* 704.

42.—A CASE IN WHICH ANEURISM OF THE ASCENDING AORTA WAS TREATED BY THE INSERTION OF WIRE.

By **Charles H. Moore**, Esq., Surgeon to, and Dr. **Charles Murchison**, Assistant Physician to, the Middlesex Hospital.

[The following is an abstract of a paper read before the Royal Medical and Chirurgical Society. The theory of the operation was first described by Mr. Moore. Dr. Murchison furnished the report of the case with medical comments upon it. For the surgical remarks Mr. Moore was alone responsible.]

In February, 1863, Mr. Moore had been led to review the conditions of such aneurisms as could not be surgically treated through the artery, and had devised a method of producing consolidation of them in accordance with the mode of their natural cure.

The principles involved in this method were—1, that large aneurisms can only be benefited by the deposition of fibrin within them; 2, that the natural means of obtaining fibrin from the blood are inadequate, because it can only settle in layers on the wall; 3, that in the central part of an aneurism there is a large quantity of blood with fibrin ready to collect on any apt material; 4, that fibrin may be elicited from arterial blood by exposing a foreign body in it. Two cases were quoted in which this had occurred; one, an instance in which gangrene of the leg had resulted from plugging of the arteries by fibrin detached from a needle in the left ventricle; the other, that of a sailor, who died three days after being shot, and in the interior of whose ascending aorta was a bullet imbedded in fibrin.

The foreign body which, according to our present knowledge, would produce least irritation was wire. If a large quantity were coiled in an aneurism, it would attract fibrin, as the twigs do in whipping freshly drawn blood, support the mass which it entangled, and lead to the cavity of the aneurism being eventually filled. The wire might be passed in through a small canula, with care not to leave the last end in the minute wound, and not to direct a coil into the orifice of the artery.

Only a sacculated aneurism could be so treated, not one which had two orifices, since fragments of fibrin would be broken off by the force of the current. Brasdor's operation might be previously required in some parts. This danger might be incurred in a sacculated aneurism also, if wire enough were not introduced, because of the large intervals which would be left between the few coils of wire. The wire would remain in the solidified aneurism, and be harmless.

Mr. Moore then described the operation by which the foregoing proposal might be carried out.

Report of a case of Saccular Aneurism of the Ascending Aorta projecting through the Anterior Wall of the Left Side of the Chest. —Daniel D., aged twenty-seven, became a patient at the Middlesex Hospital, under Dr. Murchison, on Nov. 10th, 1863. Eight years before, he began to suffer from palpitations and dyspnœa, and after some month she had an attack of hemoptysis. The hemoptysis recurred at intervals, and in Nov. 1862 he first noticed a pulsating swelling in front of the chest, to the left of the sternum. This increased with considerable rapidity, and the patient now became subject to severe attacks of angina pectoris. At the time he first came under observation, the tumour was situated in the angle formed by the left clavicle and the left margin of the sternum; it measured 10 inches in circumference at its base, and projected about 2 inches from the wall of the chest; its surface was rounded, and tolerably uniform, except at the upper part, where there was a tendency to point. Over the whole surface of the tumour distinct pulsation could be felt, each beat corresponding to the impulse of the heart. Nothing resembling an anerismal bellows-murmur could be heard; but both cardiac sounds, and particularly the second, were louder over the tumour than at the base of the heart. There was dulness on percussion to the right of the tumour, over a space measuring 2 inches transversely, and 3 inches from above downwards. The apex of the heart could be felt beating between the fifth and sixth ribs. The cardiac dulness was slightly increased, but the sounds heard on auscultation were normal. There was no inequality of the arterial pulse on the two sides of the body. The voice was normal. The patient had an occasional cough, and expectorated a viscid muco-purulent matter, but there was nothing peculiar in the character of the cough. Over the whole of the left side of the chest there was comparative dulness on percussion, with coarse, at some places tubular, breathing, and subcrepitant râle. On the right side of the chest the percussion was clear and the breathing puerile. The appetite and digestion were good, and the bowels regular. There was no dysphagia, and no pain or tenderness at any part of the spine. The pupils were of normal and equal size. On Nov. 20th, and again on Dec. 28th, the urine was ascertained to be free from albumen.

[After the patient was admitted into the hospital on the 20th of November, the attacks of angina almost ceased, yet the tumour continued gradually to increase. The integuments became of a dusky red colour, and by the early part of January it became obvious that the bursting of the aneurism could not be long delayed. With the patient's consent he was accordingly submitted to Mr. Moore's operation.]

The operation consisted in the introduction of a quantity of fine iron wire into the aneurism, with the object of inducing coagulation. A small pointed canula was inserted into the tumour, and the wire was passed in through this without difficulty. The operation occupied one hour, and the quantity of wire introduced was twenty-six yards. It gave rise to no pain or inconvenience excepting a slight and transient feeling of faintness. The quantity of blood lost did not exceed half a fluid ounce.

The immediate effects of the operation were a reduction of the pulse from 116 to 92, an almost complete cessation of the pulsation in the tumour, and a diminution in its size. Immediately before the operation, the circumference of its base was 16¾ in.; at the close of the operation it was 16 in. These changes began to be noticed soon after the commencement of the operation, and became more marked as it was proceeded with. At a quarter past eleven p.m. the patient was asleep, and his pulse was only 78. He slept comfortably during the night, and had no bad symptom until the following morning.

On Jan. 8th., at nine a.m., the patient was seized with a fit of rigors lasting three quarters of an hour, and followed by great pain in the back of the neck and some pain in the tumour. At one p.m. the pulse had risen to 144, and was full and bounding. The action of the heart was tumultuous, and all the arteries of the body could be felt throbbing with considerable force, but there was no difference in the force or volume of the beat on the two sides of the body. The patient complained of great pain in the tumour when he moved. The tumour was already somewhat larger than before the operation, and the dusky discoloration was of a deeper tint. There was intense thirst and great restlessness; the skin was dry and very hot; and the respirations were 40. At twenty minutes past one p.m. the patient was bled to the extent of eighteen ounces, and at half-past three p.m. twelve ounces more blood were abstracted. After the second bleeding twenty minims of Battley's sedative solution were administered.

From these measures the patient derived temporary relief, but he had a restless night, and at half-past six a.m. of the following morning (Jan. 9th) he had a second attack of rigors. At one p.m. he had a third attack. At half-past one he was in great distress, owing to pain in the tumour and at the back of the neck. The tumour was extremely tense and decidedly tender, particularly at its upper part. Its circumference at the base measured 1¾ in. more than before the operation. Distinct pulsation could be felt again at its upper part. The pulse was 136 and soft; the action of the heart was less tumultuous, and there was no abnormal cardiac sound. Large and repeated doses of opium and digitalis were now commenced. At half-past ten p.m. the pulse had fallen to 126, but the patient complained of being afraid to cough on account of a severe jerking pain in the tumour, which the effort to do so always induced.

On Jan. 10th, at ten p.m., the patient had taken seventy-three minims of tincture of digitalis, and the equivalent of almost ten grains of opium, during the preceding twenty-one hours, and the result was, that he was in less pain, the pulse had fallen to 104, and the tumour was slightly reduced in size, its circumference being half an inch less than on the preceding day. At half-past seven p.m., however, he was seized with a severe burning pain in the tumour, and a feeling of tightness as if it were going to burst. The tumour was larger and more tense than ever; the pulse rose to 132; the heart's action was again more impulsive, and there was intense thirst. To-day it was noted for the first time that the pulses in the right temporal and radial arteries were slightly fuller than in the corresponding vessels on the left side.

Opium in large and repeated doses, along with digitalis, was persisted with. In the course of two days and a half (commencing on Jan. 9th) as much as the equivalent of twenty-seven grains of opium was administered. The treatment, however, failed to give relief The tumour increased rapidly in size, and on the 11th distinct pulsation could be felt at several parts of its surface. The radial pulse was 128, small and compressible, and still fuller on the right side. The beat of the right anterior tibial artery was also decidedly fuller and stronger than that of the left. The cardiac impulse was extremely feeble. The respirations were performed chiefly by the diaphragm and the muscles on the right side of the chest; the left side of the chest was almost motionless. The whole of the left side of the chest in front, unoccupied by the tumour, was dull on percussion, and no respiratory sound could be heard on this side, except immediately below the clavicle. Brandy and other stimulants were now given, but without any decided result.

On the morning of the 12th the patient was evidently sinking. The pulse was about 136, but was so weak as to be counted with difficulty. The circumference of the tumour at its base was now 3½ inches more than before the operation, and urine passed during the night was found to be loaded with albumen. At eleven a.m., four days and twenty hours and a half after the operation, the man died.

An autopsy was performed a few hours after death. The walls of the external tumour were formed by the integuments and fibres of the pectoral muscle, infiltrated with serum. They were nowhere less than a quarter of an inch in thickness. The skin covering a great part of the tumour presented a deep livid hue. The interior of the tumour was filled, for the most part, with a fibrinous coagulum, enveloping and imbedded in the coils of wire, and firmly adherent to the surrounding walls. The rest of the cavity contained fluid black blood. The interior of the outer tumour was nowhere lined with a prolongation of the arterial coats; but it communicated with the proper aneurismal sac within the chest by two large openings in the first and second left intercostal spaces, the intervening rib being bare and eroded, and at one place broken through. The aneurismal tumour within the chest was about the size of a man's fist. It lay immediately behind the sternum; it encroached slightly upon the upper lobe of the left lung, and inferiorly it rested upon the right auricle. It was partially filled with a fibrinous coagulum, which was continuous with that in the outer tumour, and was adherent at one part over a space measuring about one-third of an inch in diameter. It communicated by a circular opening, scarcely so large as a sixpence, with the ascending aorta. Through this opening a clot projected from the aneurism into the vessel, and extended downwards into the heart, and upwards into the arch. The greater part of this clot was evidently of post-mortem date; but part of it, close to the opening, was pale, firm, and laminated. There was considerable atheroma of the coats of the thoracic aorta. The pericardium contained about eight ounces of turbid serum, and its opposed surfaces were coated with a thin layer of recent lymph. The upper part of the parietal pericardium presented a patch of livid discolouration, about the size of a florin; and at the centre of this patch the cavity of the pericardium was merely separated from that of the aneurism by a delicate membrane. It was at this part of the aneurism that the coagulum was adherent. The heart was slightly hypertrophied. Its valves and muscular tissue, and likewise the coronary arteries, were healthy. The left lung was everywhere firmly adherent, and its pleura much thickened. On section, numerous cavities were observed, evidently resulting from dilatations of the bronchial tubes. In the intervening spaces a firm fibrous tissue took the place of the normal vesicular structure. The right lung was for the most part healthy. Both kidneys contained a number of circumscribed abscesses, varying in size up to that of a small pea. A cluster of six of these small abscesses was found at the apex of the left kidney. In the cortical substance of both kidneys a number of patches of yellowish deposit of a large size, but less defined outline, were also observed. On microscopical examination, this appearance appeared to be due to the presence of a granular exudation deposited between the uriniferous tubes. The liver was large and fatty. The other parts of the body could not be examined.

After recording the history and post-mortem appearances of the case, Dr. Murchison enumerated some of the more important features of clinical interest, independent of the operation, which it presented.

In his concluding remarks, Mr. Moore referred first to the circumstances of the operation, and to its early effects, which were highly satisfactory. So much fibrin appeared to have collected at the end of an hour that the pulsation of the tumour and its sharp second stroke were no longer perceptible; the aneurism had much diminished in size; the pulse, which, notwithstanding medicines, had beat 112 for weeks previously, and was 120 before the operation, had fallen to 92, and at night was about 80. This unlooked-for abundance of the fibrin, accumulated not by an inflammatory, but by a mechanical process, was an unprecedented circumstance. Its effects could not be foretold. It appeared to have caused local inflammation and the rigors, with great acceleration of the pulse. No aneurism could long resist such a pulse. Death had probably resulted from acute pericarditis, which was induced, not by continuity with the inflammation outside the chest, since neither the inner aneurism nor the pleuræ were inflamed, but incidentally by the imminent bursting of the intra-thoracic aneurism into the pericardium. No fragments of the clot of fibrin, large enough to be detected by the naked eye, had been detached, but microscopic portions were supposed to have existed in the kidneys. Neither the old nor the recent disease of the kidneys had actually caused death, which was due to the changes about the chest, and was, through the pericarditis, an indirect and not inevitable consequence of the operation. No coil of wire had passed through the opening of the aneurism.

Three facts appeared to Mr. Moore to justify a repetition of the operation, or of some modification of it :—1. The separation of fibrin upon the foreign body, and its rapid deposition. 2. The exemption of the inner aneurism from inflammation, probably to be accounted for by its possessing a lining membrane, which the outer cavity had not. 3. The firm adhesion of the clot to the wall.

Not having yet thought of a more suitable foreign body than wire, he had but two modifications of the operation to suggest; 1. The introduction of a smaller quantity. The objections to this had already been pointed out. 2. The use of slender needles as temporary means of procuring the consolidation of the fibrin. The safety of this must depend on the number of needles which might be inserted, the gentleness with which they should be withdrawn, and the period at which fibrin so procured should acquire a sufficiently firm attachment to the wall of the aneurism to allow of the artificial support of the needles being dispensed with.—*Lancet, April* 2, 1864, *p.* 383.

43.—ON A CASE OF SPONTANEOUS THROMBOSIS IN THE LEFT FEMORAL AND SAPHENA VEIN.

By Dr. H. M. Tuckwell, late Radcliffe Travelling Fellow, Oxon.

[The patient was thirty-three years of age, had enjoyed previous good health, but had for some time been over-worked.]

His present illness commenced about a month ago with severe pain in the head, which lasted five or six days without intermission, and left him much weakened. The pain then shifted to the loins, and continued there for two weeks, during which time he was extremely depressed and unable to get about. At the end of this time, rather more than a week ago, he was attacked suddenly in the night by a violent pain in the right side, which seems to have been purely neuralgic, for no signs of pleurisy could be detected at the time; this pain continued for three days and nights, during which time *he lay continually on the left side*, without sleep, and taking no food, till, on the fourth day, it left him almost as suddenly as it had come on. On attempting now to change his posture he found that the left leg was quite numb, that sensation began to return after rubbing the leg for a time, but that, with returning sensation, there came on an intense pain in the lower part of the leg, more especially in the calf; that he soon observed the leg and foot beginning to swell; that the swelling gradually involved the whole leg and extended up the thigh to the groin, the pain becoming generally diffused and intensified as the swelling extended and increased. I saw him on the fourth day after the swelling had commenced, when the following appearance presented itself:—

He is extremely prostrate, unable to raise himself in bed; the eyes hollow; the voice low and changed; skin generally cool, except that of the left leg, which is abnormally hot; pulse 60, very small, thready and irregular, intermitting at every third beat; thorax generally resonant on percussion; respiration feeble, but free from any morbid sound; urine natural. The left leg, from the groin to the toes, is enormously swollen, pitting everywhere deeply on pressure, its surface is hot and very sensitive; an indistinct hardness can be felt through the œdema along the course of the femoral and saphena vein as far as Poupart's ligament, above which point it cannot be traced; beneath the œdematous integuments large superficial veins are seen ramifying along the anterior and outer aspect of the thigh.

Diagnosis.—Thrombosis of saphena and femoral, perhaps of external iliac vein.

Treatment.—To relieve the pain, cold was applied to the whole limb in the form of evaporating lotions, and the limb was supported on pillows. Small quantities of brandy and wine were administered, with milk and beef-tea.

On the day following, Mr. Savory saw the case with me, and gave me the benefit of his valuable opinion. He fully concurred in the diagnosis, but advised that the leg should be wrapped in cotton-wool, and pressed upon me most forcibly the necessity of increasing the quantity of stimulants, bidding me, to use his own words, "measure the quantity not by the glass or bottle, but by the effect produced." From this time, brandy, rum, port, sherry, and champagne were given every two or three hours day and night, till, on the sixth day from the time that I first saw him, he was taking, in the twenty-four hours, brandy, ℥xij.; rum, ℥xij.; wine, ℥xx. The effect of this on the pulse was as follows:—It rose from 60 to 94, the intermissions at the same time becoming less frequent, and the volume better and better, till, on the tenth day from the time that the stimulants were first given, and while he was still taking the above quantity, *it fell to* 84, and ceased to intermit. His general condition improved, *pari passu*, with the pulse. At the request of the patient, I returned to the cold applications, after having made fair trial of the cotton-wool, and found that it made the leg uncomfortable, while the cold relieved the pain in a marked degree. On the twelfth day, the pain having quite subsided, while the swelling remained unchanged, the leg was carefully rolled in flannel bandages, moderate pressure being at first employed and gradually increased, and it was swung from a fracture cradle, with the foot slightly raised. This was continued during a period of seven weeks, at the end of which time the swelling had entirely disappeared. It may be remarked, that the œdema subsided rapidly for the first week after the application of the bandage, but then seemed, for a time, to remain stationary, and was at last slowly removed. The quantity of stimulants was gradually reduced after the fourth week.

He has now recovered his health, can walk two or three miles in the day, but still finds, after a walk, that the leg and foot feel heavy, and that the veins in the foot become in spite of a lace-stocking, considerably distended. There is now nothing abnormal to be felt or seen in the thigh in the region of the large veins, nor is there any visible enlargement of the superficial veins there.

Remarks.—The occurrence of spontaneous coagulation in the living veins, simply as a result of nervous debility, independently of the puerperal state, of fever, or of any wasting organic disease, as phthisis, cancer, &c., is, seemingly, a rare phenomenon. On reading through Virchow's masterly paper on the subject, I cannot find, among the many cases of thrombosis there enumerated, one exactly similar; nor is there in Cohn's monograph one case in which there was not either some organic disease or fever to account for the coagulation. Not that I wish to instance this case as one whose pathology is distinct or special, for the so-called "Marantischer Thrombus" of the Germans, or clot that forms in wasting diseases, is, as Virchow has shown, dependent primarily on the same cause,—an enfeebled state of the heart's action; but the disease here presents peculiar interest, in that there was an absence of any dyscrasia or fever which might be supposed to give rise to the formation of a thrombus by altering the composition of the blood; it shows that to a feeble heart alone may be attributed all the symptoms and signs of the worst form of "phlegmasia dolens." It may be urged by some, in contradiction of this assertion, that phlebitis was here the real cause of the coagulation; but a careful observation of the way in which the disease showed itself seems to me to afford convincing evidence that no phlebitis whatever was present, and thus to confirm still further the doctrines of Virchow. The rapid development and extent of the swelling certainly point to a primary obstruction of the main venous trunk in the thigh. Now, if this obstruction had been due to phlebitis, surely the first symptoms of pain and swelling would have been noticed in the immediate neighbourhood of that venous trunk, whereas nothing of the kind was observed. The swelling and pain commenced in the leg and foot, and extended, last of all, to the thigh; nor was there at any time marked pain, along the course of the obstructed vein, distinct from that felt all over the leg. Besides, the general symptoms were not those of an acute inflammatory process; the skin of the body, generally, was cool, and the pulse quite unlike that of inflammation. The conversion of the saphena and femoral vein into a solid tube by coagulation of their contents, is quite sufficient to account for the hardness felt along their course.

John Davy and Gulliver were the first to notice the frequent occurrence of clots of this kind in the veins of those who had suffered from chronic diseases, with failing circulation and great prostration of the vital powers. After them, Hasse and Bouchut turned the attention of pathologists still further in this direction. But to Virchow must be awarded the largest share of praise, for he it was who first cleared away the mists which enveloped the whole subject; he showed, by repeated experiments and post-mortem examinations, that the doctrine of phlebitis, first promulgated by John Hunter, is erroneous; that the coagulation is not preceded by inflammation of the vein, and that there is no exudation on the free surface of its inner coat which determines coagulation; but that, through failure of the heart's power, the blood current is retarded and finally stagnates, and that the starting-point for coagulation is at the point of junction of the valve with the wall of the vein, the valve here (like the chordæ tendineæ in the heart) acting as a foreign body, and furnishing a centre round which the stagnant blood coagulates.

It is worthy of notice that, in circumstances predisposing to thrombus formation, a long continuance in one posture seems to favour the occurrence of the phenomenon, and that the side to which the patient inclines is often the side on which the clot forms. Virchow dwells upon this, and relates cases which corroborate the statement. Cohn has observed the same; he mentions one case in particular—a case of Bright's disease with effusion into the right pleura—where the patient lay continually on the right side and right arm, and where the whole right arm became œdematous from the formation of a thrombus found after death in the subclavian vein.

The contingencies to be feared in these cases are, first, that the heart may not be able to recover itself, and that death by asthenia may follow rapidly; secondly, that erysipelas may set in, followed by diffuse abscess, or even gangrene; thirdly, that a portion of the clot may be detached and washed into the pulmonary artery, causing sudden death; fourthly, that the clot may undergo the so-called retrograde metamorphosis, may soften and break down in its interior, and that this softened, ill-conditioned fibrine may be carried into the general circulation, and cause death from pyæmia. The heart must regain its lost power, and the clot must undergo a healthy process of organisation, before anything like a favourable prognosis can be given.

The question then arises as to how the circulation is re-established; the answer to which is, by the formation of collateral channels, if the vein be completely obliterated by the organised clot. But it may also happen that the canal of the obstructed vein may in part reopen; that the clot during the process of organisation, may shrink away from one wall of the vein as it becomes adherent to the other; and that the blood may flow on again in its original channel, now of necessity much narrowed by the changes that have taken place. It is probable that the latter has occurred in the case related, from the fact that there are no large superficial veins visible.

The circumstances of principal interest in the treatment employed are, the beneficial effect of cold, the value of pressure, and the necessity of stimulants.

The application of cold—a remedial agent so largely employed in Germany—is strongly recommended by Virchow as the best and often the only means of alleviating the terrible pain that follows the sudden obstruction by an embolus of one of the large arteries of the extremities. The relief it afforded in this case was most marked. Pressure carefully applied and gradually increased by means of a flannel bandage—a plan of treatment employed by many obstetric physicians in the later stages of the puerperal phlegmasia dolens—was here, too, attended with good results. The rapid and manifest improvement in the general condition of the patient, and the restoration of the heart's power in proportion as the quantity of stimulants was increased, sufficiently indicate their importance in the treatment of such cases. And, surely, if alcohol acts thus beneficially after the mischief has been done, we cannot avoid the reflection, that the free use of alcohol in wasting diseases may often avert the mischief altogether, and that the stimulant plan of treatment has, at any rate, this much to be said in its favour, that it tends to prevent the formation of thrombi. In the history of this disease we have, certainly, one satisfactory example of the way in which a real advance in pathology leads to a corresponding advance in therapeutics.—*Medical Times and Gazette, Feb.* 13, 1864, *p.* 170.

ORGANS OF RESPIRATION.

44.—ON THE IMPORTANCE OF EXCISING A SMALL PORTION OF THE WINDPIPE IN THE OPERATION OF TRACHEOTOMY.

By George H. Porter, Esq., M.B., T.C.D., Examiner in Surgery, Royal College of Surgeons, Ireland.

[In many cases of tracheotomy a tube cannot be endured from the amount of irritation it occasions. It is in these cases that Mr. Porter advises the excision of a small portion of the trachea. A longitudinal incision of about half an inch must be made in the middle of the trachea, and a thin slip of the tube removed, leaving an opening about equal in size to the natural one.]

The late Mr. Carmichael strongly advised the excision of a diamond-shaped portion of the trachea, as forming a free exit to viscid phlegm, generally present in those cases, and at the same time objects to the canula, on the ground that "it excites intolerable irritation, with violent fits of coughing." Pelletan, and many others, condemned the canula as being insupportable to the patient, and adding to the irritation. I cannot, however, go the entire length with those who would assert that this appliance usually produces a large amount of distress. I have frequently employed it myself successfully, and seen it introduced by others with the most happy results; but, on the other hand, I have certainly witnessed instances where its presence could not be endured for a moment. I have notes of two cases in which I performed tracheotomy—one, that of a girl, aged sixteen years, labouring under acute laryngitis; the second, where a man of forty-three suffered from syphilitic ulceration of the larynx. In neither of these cases could the tube be borne, even for an instant—a fact which, at the time, impressed me very strongly with the necessity of cutting out a small piece of the windpipe instead of making a mere linear incision. I shall not, however, dwell upon this point, as my object is not to discuss the propriety of using or not using a canula. I am perfectly aware that it cannot be dispensed with in many cases, and therefore it should always be at hand when we are obliged to open the trachea.

With regard, however, to the main object of this paper, let me not for a moment be considered as condemning the practice of others, but merely as endeavouring to impress on the operating surgeon that which I deem a most important feature in tracheotomy, and which I myself have found to possess surpassing advantages. In a word, I believe it to be of great importance that a portion of the tube should be excised, though I am quite aware this course has not been generally sanctioned. I do not presume to put this forward as any original conception of my own, for, among those in favour of such a plan of opening the windpipe in certain cases, I may mention my late father, as stated in his work on the larynx and trachea, published in 1826; Professor Hargrave, who also recommends such a procedure (Operative Surgery, 1831); Ryland, who advises it under special circumstances; Mr. Fergusson, who says:—"If the opening has been made to allow the patient to breathe more freely, it will be necessary to make some provision to keep it patent. Some portions of its margins may be cut away with this object." Mr. Lawrence and the late Mr. Carmichael (as before quoted) practised this same method. The advantages arising from the excision of a portion of the trachea may be briefly detailed. In the first place, I consider that it allows an easier introduction of the canula than when it is inserted through the mere incision. Secondly—it favours a more free escape of mucus, flakes of lymph, or blood, from the interior of the windpipe, which is a most important result to be aimed at. Thirdly—when the canula is introduced through an opening of its own size, and which it fits accurately, there is not the same likelihood of blood passing into the trachea as there would be in the case of a slit. The slit, when its edges are separated by the canula, must leave a triangular opening above and below the instrument. Fourthly—in cases where the presence of the canula cannot be tolerated, from the irritation it gives rise to, it can be well understood that such an opening is absolutely essential to permit free respiration. Fifthly—should tracheotomy be performed for the removal of a foreign body, it will be quite apparent that the aperture made by cutting out a portion of the windpipe would give a much greater facility for the introduction of instruments to accomplish its dislodgement. Sixthly—if a canula be placed in the trachea it would be much more likely to remain in its proper position when fixed in an opening which approximated to its own size and shape than if inserted through such a slit as that objected to. Seventhly, and lastly—in cases of suspended animation the free opening made by the removal of a piece of the windpipe would allow the application of the nozzle of a bellows, or the passage of a suitable tube, much more easily than a simple incision.

But in carrying out the proposed method it is possible to run into an opposite extreme, and thereby commit a grievous error; for we may cut out too much of the tube, and thereby expose the patient to serious suffering. And hence, as previously admitted, objections have been raised to it. Ryland, in his admirable work on the Diseases of the Larynx, thus alludes to it in disparaging terms:—"The removal," he says, "of a portion of the front of the trachea is by no means unattended with inconvenience. For, in the first place, when the operation is performed at the lower part of the neck, the distance between the surface of the integuments and the trachea is so great, that it becomes a matter of considerable difficulty to cut away an elliptical portion of the tube.

"The difficulty amounts almost to an impossibility when the integuments are much tumefied either by emphysema or by serous or purulent infiltration. This step of the operation is, however, greatly facilitated by drawing forward the trachea by means of a hook inserted underneath one of its cartilaginous rings. In the second place, the removal of a portion of the front of the windpipe, especially in children, may probably occasion a narrowing of the tube at this part to such an extent as subsequently to interfere with the functions of the organ. The area of the tube is, in the natural condition of the parts, preserved by the cartilaginous rings, which counteract the disposition of the muscular bands at the back to diminish its calibre. But when the centre of two or three of the rings is removed to any extent, the action of the muscular fibres remains uncontrolled, and the trachea becomes, in consequence, very much narrowed."

The late Mr. Liston, on the same grounds, was unfavourable to the removal of any of the windpipe, and expressed his opinion as follows:—"It has been proposed to remove an oval portion of the tube. This is a difficult matter, does not answer the purpose long, and is apt to be followed, after the healing of the wound, by inconvenient and dangerous narrowing of the trachea."

Now, respecting the first objection urged by both authors, I find it hard to understand it, particularly as stated by the latter surgeon, who was universally acknowledged, in his day, to have been a most expert and daring operator. I should have thought that the man capable of passing a catheter with *one* hand could as easily cut out a piece of a trachea (properly bared) as make an incision in its centre line. I can fully understand the validity of the latter objection if too large a portion happened to be taken away, especially in the case of a child. In any operation, however, the surgeon may use his knife too freely, as well as in this; but why argue from the abuse against the proper amount of excision? I have never witnessed an instance of this dreaded narrowing of the windpipe when tracheotomy was performed in the manner which I do not hesitate to recommend. In the case of a young girl I cut out a larger portion than I intended, and she perfectly recovered, and is now in the enjoyment of the best health, after a lapse of nine years, never having suffered for a moment during that period from any laryngeal affection. I may here, also, give the experience of Mr. Wells on this subject, as set forth in his excellent article on "Bronchotomy" in The Cyclopædia of Practical Surgery. He says:—"As to the danger of narrowing, I have not read of any cases supporting this view. On the other hand, I have, of many, where a portion was removed, and the aperture is said to have been filled with a ligamentous substance, and no narrowing occurred."

Some very ingenious instruments have been invented and suggested to the profession for making an opening in the trachea; and one for cutting out a piece from the front of this tube. Among the former I may mention that of M. Gerson, of Hamburgh; Thompson's Tracheatome; the Forceps of Garin; Edwards' Hook; and the Trochar and Canula. This last-named instrument is a most useful one in cases where bleeding occurs, and which will not admit of delay in opening the air-tube. But there is one great disadvantage attending it; namely, if the canula, on the withdrawal of the trochar, produces the violent irritation sometimes seen, its removal becomes imperative; and then, instead of leaving a circular or oval patulous wound, it is found triangular, with closing flaps, which afterwards prove troublesome. There is only one instrument that I am aware of for *cutting out* the portion of the trachea. Its invention is claimed both by Mr. Milliken, formerly of Dublin, now resident in London, and by Dr. Marshall Hall. It is very ingenious, and cuts out the piece by a rotatory motion when fixed in the trachea, by means of a screw. I have never seen it used; but I am of opinion that the motion of the windpipe, and the fact of the cutting edges being applied to a convex surface instead of a plane one, must render its operation more or less tedious. The best mode of proceeding, in my judgment, is, to remove a circular portion with the knife, adapting it as nearly as possible to the size of the canula; and, although in some instances the operator may not be successful in attaining the precise size he desires; yet, in general he will be enabled to perform the operation sufficiently exact to secure all the objects desired. The form of knife that I have several times used and recommended for this purpose, I first saw employed by my father. It is a sharp-pointed "tenotome," with a narrow, but strong blade. Having fixed the trachea by catching it with a hook, the knife is carried round the portion of windpipe, firmly held forward, and a piece excised. Thus an opening is formed which freely admits the introduction of a canula, if required; but, on the other hand, should its presence not be tolerated, an ample aperture is left for the uninterrupted admission of air.—*Dublin Quarterly Journal, Feb.* 1864, *p.* 6.

ALIMENTARY CANAL.

45.—ON SYME'S OPERATION FOR THE RADICAL CURE OF REDUCIBLE HERNIA.

By George H. Porter, Esq., Examiner in Surgery Royal College of Surgeons, Ireland.

When Professor Wutzer, of Bonn, suggested his operation for the radical cure of hernia it promised fairly to fulfil all requirements. Surgeons hailed it as one of the great *desiderata* in their art, and hoped that a remedy had at last been found to enable them to dispense with the galling truss. Now, we must remember that in any operative measure for the permanent cure of a rupture, the obliteration of the sac should be effected, as well as the contraction of the hernial aperture. Wutzer's apparatus certainly accomplishes much towards the desired end, by inserting a plug of skin and other textures to fill the deficiency in the abdominal walls, and thus prevent the escape of intestine. The operation is not painful, and is comparatively without danger to the patients; but, although one of the best plans, it may be objected to on some grounds. In recent ruptures, for example, when the opening is not large, the insertion of the wooden cylinder is calculated to widen the ring, and thus, by tearing asunder the parts, to produce the very result which is intended to be obviated. I am not now, however, about to discuss the merits of operations performed for this purpose, some of them proposed and executed by surgeons of the highest ability; but I merely wish to give my own experience of one which was simple in its performance and most happy in its consequences. I allude to that recommended by Mr. Syme in his *Observations in Clinical Surgery*.

On the 21st May, 1863, a man, aged thirty-five years, a carpenter by trade, was admitted under my care into the Meath Hospital, with a reducible oblique inguinal hernia on the right side, of which he had become the subject, three months previously, whilst lifting a weighty piece of timber. The tumour was not large, nor had it passed beyond the external ring. He suffered pain in coughing, sneezing, or when he made any unusual exertion at his trade. He was at first quite ignorant of the nature of his malady; but, when informed that it was a rupture, he begged that anything might be done which would secure him from the annoyance of wearing a truss, of the discomfort of which he had often heard people speak. Considering his age, good state of health, the small size of the tumour, and the length of time he was suffering from the disease, I regarded it as a favourable case, of which a radical cure might be attempted. Before describing the operation, I cannot do better, perhaps, than quote from Mr. Syme's work respecting the instruments and appliances required. He says :—"Instead of a complicated machine for distending the invaginated integument, I employed a piece of bougie, or gutta percha, to one end of which was attached a strong double thread. The plug, thus prepared and smeared with cantharides ointment, was drawn into its place by the threads, which, by means of a curved needle, guided on the fingers fairly within the ring, were passed, at the distance of rather more than an inch from each other, through all the textures to the surface, where they were tied firmly together on a piece of bougie, to prevent undue pressure on the skin."

Operation.—May 27th. The patient having had his bowels well freed by castor oil, taken the night previously, and his bladder emptied a few minutes before, was placed in the recumbent position on a table, with his legs hanging over, his feet supported, his shoulders slightly raised, and the right thigh flexed a little. Standing between his legs, I pushed up, with the forefinger of my left hand, a large plug of scrotal integument into the external abdominal ring; and, feeling fairly within its sides, I took the needle with one of the threads attached, and, guiding it along the finger so placed, I transfixed the external pillar and abdominal parietes. Then, taking the thread from the needle, an assistant threaded it with the second cord, when I passed it through the internal pillar, and made its point appear about an inch and one eighth internal to the first aperture. Having then withdrawn the finger I pulled the portion of bougie, well smeared with cantharides ointment, by means of the two strings, into the situation previously occupied by my finger, invaginating the structures as it assumed its place; and, lastly, I tied tightly the two threads across the second piece of bougie, to prevent any injury to the skin by pressure. The patient suffered scarcely any pain, wincing merely a little at each puncture of the needle. He was placed in bed after the operation, with the right thigh elevated slightly on a pillow, a small pledget of wet lint laid over the site of the ring, and a full opiate administered.

May 28th. Had slept well; in no pain; a faint redness visible about the needle-punctures.

May 29th. Rested easily; free from pain; and no tenderness whatever on pressure in the abdomen. He says that when he coughs he feels, to use his own phrase, "quite tight below." It would be tedious to give the details of this case from day to day. It will suffice to say that he went on as favourably as I could wish in every respect, and never had a bad symptom. I removed the bougie on the 7th June, and found that the punctures were slightly ulcerated near the threads, with a blush of redness about them. The site of the external abdominal ring presented a round swelling, the size of a large nut, and was very hard to the touch. This appeared to be formed by the invaginated structures and lymph thrown out around the parts. The man coughed forcibly without in the least displacing the plug of skin, which was now firmly lodged in its new position—while there was some discharge from the excoriated surface inside the inverted integument. I then laid a compress of lint over the part, and applied a spica bandage. He was thus kept confined to bed until the 20th June, when I allowed him to walk about the ward with the bandage on. June 22nd I tested the result of the operation by making him cough violently and jump from a chair to the ground several times; but notwithstanding this exertion not the least protrusion occurred, and the ring appeared permanently sealed up. I put a truss on him with a very light spring, and permitted him to leave the hospital on the 25th June—desiring him to wear it during the day, to avoid any great straining or exertion as much as possible, and to let me see him occasionally. I saw him very recently; and the disease is, to all appearance, cured. He is able to follow his trade with comfort, and feels the part as strong and unresisting as any other portion of the walls of the abdomen.

I have performed Wutzer's operation for the cure of hernia three times, and have seen Wood's procedure done twice, and, as far as my opinion goes, must give the preference to this of Mr. Syme. I cannot conclude better than by repeating the three advantages it possesses, as stated by that able surgeon:—"1st. That it may be executed by means which are in the possession of every surgeon, instead of the complicated, expensive, and not easily manageable apparatus, hitherto deemed indispensable. 2nd. That it may be accomplished with much more certainty through the secure guidance of a finger, than by trusting to a piece of wood, for gaining admission within the tendinous ring. 3rd. That the two threads, co-operating in their effect, render the chance of adhesion between the textures much greater than when it is attempted by the mere puncture of a needle."—*Dublin Quarterly Journal, Nov.* 1863, *p.* 265.

46.—ON TRUSS PRESSURE IN INGUINAL HERNIA.

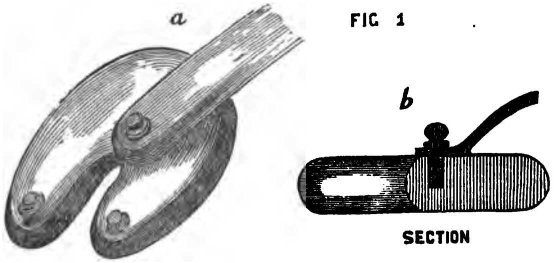
By John Wood, Esq., Demonstrator of Anatomy, King's College, London, and Assistant Surgeon to the Hospital.

On dissecting the coverings of a hernia of whatever size, it will be found that their weakest part, both behind and in front, is that opposite to the long axis of the hernia or inguinal canal. If, consequently, it is attempted to prevent the entrance of the bowel into the sac by the pressure of a convex surface, such as is found in most trusses in common use, the effect is simply to reverse the action of the bowel on the front walls of the sac, and invert its convexity into the canal, and, at the same time, to perpetuate or increase the injurious backward pressure upon the already weakened posterior wall of the inguinal valve.

The theoretic perfection of a truss pad would be one which would fulfil four indications—viz., 1st. To oppose resistance to the first egress of the bowel through the deep ring by flat pressure over its site. 2nd. To press firmly upon the outer pillar and Poupart's ligament, so as to prevent their yielding before the tumour. 3rd. To press firmly and independently upon the upper wall of the canal, lying behind and above the inner pillar; and, lastly, to leave the axis of the canal and the superficial opening uninvaginated and not stuffed out by the front coverings of the rupture.

The ordinary neglect of these indications has led to the practice of putting on convex truss pads, under the use of which the hernial openings are continually widened by invagination of the skin and front coverings of the hernia, giving rise to a gradual increase of the rupture in size as noticed each time it is suffered to come down. The constant wearing of the truss, with the view of producing a cure by gradually lessening the rupture and closing its openings, thus produces the very opposite effect, of enlarging them. This effect is much increased by the constant boring or lateral motion of the side spring during walking in those trusses which have no joint between it and the pad.

FIG 1



To meet these requirements, I devised a truss-pad for oblique hernia, having a perfectly flat surface, rounded off at the edges, and arranged in the shape of an oblique horse-shoe, with one end longer than the other (see Fig. 1). The bow of the bend is placed over the deep opening of the rupture, and the two ends over the two pillars of the ring, respectively following their several directions and obliquities. The cleft or fissure is placed over the axis of the canal and superficial ring. This truss-pad has the additional advantages of permitting the cord to emerge over the pubis without being subjected to pressure, by placing it within or under the cleft. The ends of the pad are also placed on each side of the pubic spine, so as to avoid a source of annoyance resulting from chafing of the skin over this bony projection, which is so commonly experienced in wearing the ordinary pads.

For direct hernia, also, I have devised an ovoid ring pad, with a flat bearing margin, intended to surround the margins of the direct opening, and to prevent their dilatation without invaginating the front coverings of the rupture into the hernial aperture (see Fig. 2). The hole in this pad (like the cleft in the horse-shoe) permits of a certain degree of projection of the skin into it, sufficient to fix and hold it from shifting under the pressure of the side spring or belt, without at the same time permitting the sac or bowel to project, for which a much wider interval would be necessary. These pads, in fact, effect as nearly as mechanical appliances can do, what the fingers of the surgeon do when he is holding up the rupture of a patient without invaginating the sac. For ordinary purposes, these pads may be made of box-wood well smoothed off. I have now a great many such in use, both upon patients who have been operated on for the radical cure (after which I consider their temporary use to be essential to support the newly-formed adhesions, without causing their absorption), and upon others who have objected to an operation, or in whom I have considered it unadvisable. They keep their place quite as well as the common pads, and have kept up ruptures which no other truss has been able to accomplish.

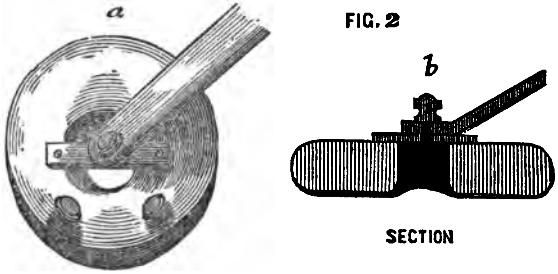


FIG. 2

SECTION

An india-rubber water cushion may be fitted to the surface of these pads, which will relieve the skin under their immediate pressure in cases where this is necessary. Mr. Matthews, of Portugal-street, has fitted some for me latterly, which have been very satisfactory. In infants, the pad so protected may be fastened to the body and thigh by an elastic figure of 8 band, of sufficient power to retain the rupture without being liable to shift or to interfere with cleanliness or the movements of the child.

In special cases I have had recourse to a lever spring within the pad itself, acting by two movable ends at the lower part upon a joint at the upper part, in conjunction with the pressure of the side spring or belt. By means of these pad springs we obtain an increased pressure at the lower end of the pad, which enables us to overcome some of the difficulties arising from a protuberant abdomen or projecting hips, as well as those resulting from a peculiar direction of force in the rupture itself. These pads have been worn in many very difficult cases with the best possible results. In some adult cases I have watched, a decided diminution of the size of the rupture has ensued.

It is, however, a matter of common remark, that few cases of cure, comparatively with the number of cases and trusses worn, have resulted from treatment by pressure only, even in the young, and how very rarely such a result ensues in the adult ruptures. In difficult and large cases I have employed a truss-pad split into two halves for independent pressure upon the two pillars. Each half is acted upon by the separate ends of a spring going round the hips like a double truss. The twin halves are connected with each other by an elastic or leather strap.

It has, therefore, long been a desideratum in surgery to obtain a method of procedure at once safe and efficient to effect a radical cure of this very common and serious deformity. If by a radical cure is meant such a closure of the groin as to render the production of a rupture on that side ever afterwards impossible, then such an effect is no more within our reach than that the cure of an aneurism guarantees against any more aneurisms on the artery operated on; or an operation for urinary calculi against the formation of more stones; or amputation or excision for diseased bone against any more diseased bone; or dividing tendons for deformities against the return, in a certain percentage of cases, of the same, or a like distortion. But if it is meant by a radical cure, that the parts shall be rendered more secure against rupture than the same parts were before the rupture, in a proportion of cases quite as great as the success in most surgical operations, then I conclude (from the experience of nearly one hundred cases during the last six years) that, by the operations which I propose to describe in future papers, this great desideratum may be arrived at with as little or less risk to life or limb as in any surgical procedure of like importance.—*Medical Times and Gazette, Feb.* 27, 1864, p. 223.

47.—ON THE OPERATION FOR THE CURE OF RUPTURE.

By JOHN WOOD, Esq., Assistant-Surgeon to King's College Hospital, London.

The consideration of the causes and progress of inguinal hernia will render it evident that, to produce such a closure of the hernial opening as shall prevent either sac, bowel, or omentum from escaping, it will be necessary so to close the tendinous sides of the inguinal canal to such an extent as to restore, if possible, the valve-like action which it exerts in restraining the egress of the viscera. It is evident that this cannot be done by dilating the openings by a hard plug of wood, as attempted in Wutzer's and Rothemund's methods, nor by invaginating a portion of the skin of the scrotum, as in Gerdy's, nor by merely procuring adhesion of the pillars of the superficial ring, nor obliterating the cavity of the sac by seton, or injection, or by the various other methods which have been tried and failed ever since surgery became an art. To make the parts strong, resisting, and permanently secure, the aponeurotic passage must be closed entirely up to the deep ring. To do this with safety, so as to guarantee the patient against the severe consequences which often ensue from a direct opening into the peritoneal cavity, such as followed the operations of Schmucker, Langenbeck, Abernethy, and Sharp, it is necessary to give the operation a subcutaneous character.

From the experience of some surgeons, resulting in the failure of almost all the cases where the operations were slight, and in a great fatality where the operations were severe, many surgeons have been led to doubt whether it be possible, consistent with the safety of the patient, to deal with the hernial structures so effectively as to produce a complete and permanent cure.

Direct and positive personal experience of nearly 100 cases, with the result of more than two-thirds of cures, and only one death (from pyæmia), and treated mainly in a public hospital where the greater part of the cases have been from time to time publicly exhibited during the last six years, enables me to speak positively as to both the facility and utility of the operations which I propose to describe.

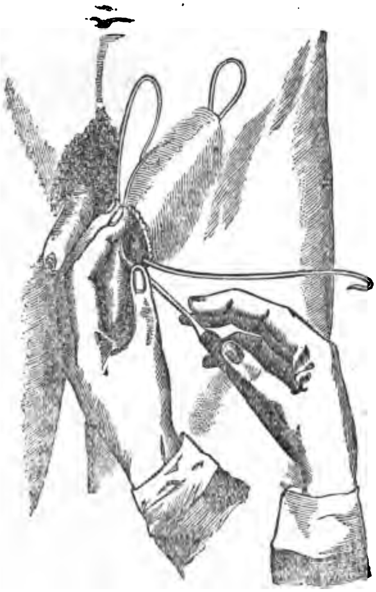
In the first twenty cases, I used for the purpose of drawing together the sides of the canal, ligatures of stout waxed thread tied over a compress of wood. This, though presenting certain advantages in its flexibility and ready management, I afterwards discarded for stout copper wire silvered over. This substance I have undoubtedly found to give rise to less suppuration and more indurative action, to be less likely to strangulate the parts over much, and particularly to give the power of applying a proper and beneficial amount of pressure upon the deeper parts of the canal during the time that the wires remain in situ.

I now practice two operations for the radical cure of hernia. In both, metallic substances only are used. One operation is for large cases occurring in the adult in which the hernial openings are so large that its sides cannot be approximated without so much strain as to cut out the ligatures before they have effected all the purpose required. In such cases it is advisable to invaginate so much of the fundus of the hernial sac and its coverings as to fill up the openings completely, and to tie upon it the sides of the hernial canal, so as to procure a complete adhesion and blending together of all these structures. The skin itself is not invaginated, because, its surfaces not being capable of adhering together, the hollow glove forming the invagination serves no other purpose than to keep separate the pillars of the superficial ring and to act as a dead weight or drag upon the parts which may have become adherent above, so as gradually to draw them down and reproduce the rupture. This is what generally occurs after Gerdy's and Wutzer's operations have been performed.

The other operation I employ for smaller cases, and those which are congenital or occur in children and young adults. It consists simply in skewering or trussing together the sides of the canal, along its whole length, by rectangular pins, which are then connected together at each end by loops, and thus are made to compress the canal and sac subcutaneously until the sides have become adherent. This operation is in itself so simple, the after-pain and inconvenience to the patient so slight, and any symptoms so rare, that, after upwards of forty trials of it, with only two failures, I am inclined to place the greatest confidence in it, when properly and carefully done, in those cases which are suitable for its performance.

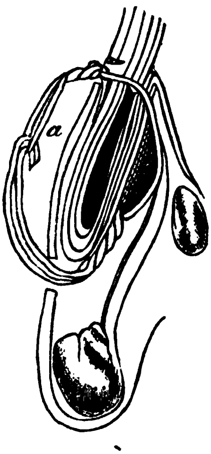
The Operation by Wire.—The scrotum and pubis being shaved, the hernia carefully reduced, and the patient placed under chloroform to such an extent as to overcome voluntary motion in some degree, as well as sensation, an inch-long incision with a small tenotomy knife is made through the skin of the scrotum, about two inches below the pubic spine, and directed obliquely downwards and outwards. The skin is then separated at the sides of the incision from the deeper fascial tissues to the extent of an inch on each side. This is done by using the point of the knife flatwise. The fascia thus detached is then invaginated into the abdominal ring and canal as far up as possible, upon the forefinger of the operator, the cord being pushed to the outer side. The lower border of the internal oblique muscle is then felt for, and hooked forwards upon the finger. This brings into prominence at the inner part of the deep wall of the canal the edge of the conjoined tendon. A stout, equally-curved needle, mounted on a strong handle and with a blunt point, is then oiled and carried along the inner side of the finger and made to transfix the conjoined tendon and internal pillar of the superficial ring from the deeper surface. When the point of the needle is seen to raise the skin of the groin, the latter is drawn over by an assistant towards the linea alba before the needle is pushed through it. One end of a well-oiled piece of silvered wire, about two feet long, is then hooked on to the eye of the needle and drawn back with it into the scrotum with a slight jerk. The needle being detached from the wire, the finger is again passed into the canal and placed behind Poupart's ligament near its middle, this structure being well raised up from the deeper parts, and the cord pushed over to the inner side of the canal. The needle is then passed along the outer side of the finger, and made to transfix the middle of Poupart's ligament from behind. When seen to raise the skin, this latter is drawn outwards by the assistant until the point of the needle can be passed through the hole in the groin before made. The opposite end of the wire is then hooked on to the needle and drawn through into the scrotum, leaving a loop of wire protruding at the upper puncture, the needle being then again disengaged. Next, the sac of the rupture within the scrotal incision is pinched up between the finger and thumb

FIG. 1.



(see Fig. 1), which are made to separate it from the vas deferens and spermatic cord in the same manner as in taking up varicose veins of the scrotum. The needle is then passed across the sac close in front of the spermatic cord. The inner end of the wire is then hooked on to the needle and drawn through across the sac after the needle. The ends of the wire are then twisted down into the scrotal puncture and cut off to about four inches in length. Traction is next made upon the upper loop, by which the lower twisted ends of the wire are drawn into the canal, invaginating the twisted sac up to the deep ring. Three or four firm twists of the wire loop are sufficient to retain the sac in this position, and to draw tight upon it the sides of the inguinal canal. The loop and ends of the wire are, lastly, connected together over a pad of rolled lint, placed at *(a)* Fig. 2, between the the wire and skin, and held steady by a spica bandage and compress.

FIG. 2.



During the operation the knees of the patient must be kept well together, and flexed upon the trunk so as to relax the structures in the groin. He must be put to bed in the same position, with the knees and shoulders raised upon pillows.

The first night or two he will usually require a sedative draught, but the necessity for this varies in different patients. If the patient's bowels have been opened by castor oil before the operation, no anxiety need be felt on their account during the first week, when the diet should be milk, eggs, and beef-tea, or a chop after a few days if the patient wishes for it. Pain usually passes off entirely after the first or second day. About the third serum is discharged through the wounds. A little suppuration may then make its appearance, and about the same time the canal becomes distended by a solid effusion of lymph, forming a hard tumour up to the deep ring. In large cases that part of the sac which remains below the wire in the scrotum may suppurate and prolong the cure. In general, however, the wire may be removed about the tenth day. As a rule, the larger the rupture the longer the wire should be kept in. If suppuration be at all free, the unyielding wire forms a very good conductor for its free escape. In some very large cases I have kept in the wire for a fortnight or three weeks.

Very large cases of direct hernia require a somewhat more complicated arrangement of the wire. Thus, I have made the wires cross each other inside the canal, and by applying the needle a fourth time at the scrotal incision have obtained another hold upon the inner and outer pillars of the ring close to their insertion upon the pubis. By this means a large hernial opening is closed by a couple instead of a single stitch, and guarded more effectively against a reprotrusion of the rupture close above the pubis and behind the upper suture. This is especially required in direct cases, whether originally so or converted from oblique hernia. These cases do very well generally, though the treatment is sometimes more prolonged than usual. In every case either a complete cure or a more effective support by a truss has been the result.—*Medical Times and Gazette, March* 26, 1864, *p.* 336.

48.—ON A NEW FORM OF TRUSS.

By T. P. Salt, Esq., Birmingham.

[Mr. Salt exhibited this truss lately at a meeting of the Royal Medical and Chirurgical Society of London. It is not described, but it is stated to possess the following advantages:]

1. It affords, in addition to the usual *inward* pressure of ordinary trusses a second or *upward* (lifting) pressure, always required for support of hernia, and attempted to be given by a variety of clumsy contrivances, the chief of which is the linen strap from behind fastened upon a peg at the lower part of the pad. This is the only truss which gives the lifting pressure in an unobjectionable manner.

2. By the peculiar mode in which the vertical or lifting springs are attached to the pads, no displacement of the pads can take place when once put in position; neither can there be any friction on the skin over the hernial openings.

3. The facility with which the position of the pads may be placed in relation to the median line of the abdomen, and the spring across the abdomen having curves coincident with it, the spring of the truss lies more evenly against the body.

4. The back support, consisting of two pads instead of one—one placed above the horizontal line (spring), the other below—gives greater stability to the front part on which the pads are suspended; hence this truss is more likely to remain *in situ.*

5. Every part of the patent truss is made by machinery, instead of by hand; hence any portion damaged or soiled can be rectified without the necessity of sending the truss to the instrument-maker.

6. The pads are an eminent improvement. The fringed edges of ordinary trusses, in which the cushions are made by stitch-

ing, are constantly chafing the skin; but by turning these cushions into a metallic capsule or clip, the edges and surface of the pads are exquisitely smooth and soft.—*Lancet, Dec.* 5, 1863, *p.* 653.

ORGANS OF URINE AND GENERATION.

49.—ON LITHOTOMY.

By Dr. G. M. Humphry, F.R.S., Surgeon to Addenbrooke's Hospital, Cambridge.

[When first appointed Surgeon to Addenbrooke's Hospital, Dr. Humphry carefully studied the operation of lithotomy, by reading and practising on the dead. He thus laid down a plan for his future guidance, and to this plan he has adhered in every case since.]

There is nothing new in the plan. The chief features of it are—first, to cut as little as possible of the deep structures (a rule applicable to other operations as well as to lithotomy); secondly, to use all possible gentleness in every stage of the proceeding. The most successful operator I had seen was peculiarly gentle in his manipulations. So far as I could gather from those who had seen him operate, this was also the great secret of Martineau's success. My impression, too, derived from the same source, is that he usually cut the neck of the bladder very sparingly, or not at all. I admit that free incissions make the operation quicker and more easy, facilitating the introduction of the forceps and the seizing and extraction of the stone; but I am confident that they make it more dangerous, increasing the risk of hemorrhage and of diffuse suppuration in the pelvic tissue. Free incisions are advocated because they diminish the necessity for rough, forcible dragging at the stone, and consequent tearing and bruising of the structures; but it is surprising how much the structures will yield, without tearing or bruising, while the stone is being slowly and cautiously drawn away. It may be a question which is the worst, free use of the knife or rough use of the forceps. The plan I advocate is that of using the knife little and the forceps slowly. By far the greater number of operations may be conducted on this plan; and I never depart from it unless compelled by the size of the stone or other unusual circumstance.

I perform the common lateral operation, using a curved staff, with the groove on its convexity, not to one side, and a common scalpel, and hold this as it is usually held in other operations. I cut freely and quickly at first till the staff is reached, and then proceed slowly and carefully, introducing my left fore-finger after each incision, so as to ascertain how much has been cut, and sometimes do this several times before the bladder is opened. When the finger can be passed into the bladder with a little force I press it on beside the staff, stretching or tearing the prostate gently as it goes; feel and examine the stone; and, the staff being now withdrawn, dilate the prostate further if necessary, or, if the stone proves to be large, incise it cautiously with a straight blunt-pointed bistoury passed along the finger. My finger is long and rarely fails to reach the stone, and I do not withdraw it till the forceps are lodged in the bladder. As the opening into the bladder is small and occupied by the finger, I have to be careful that the forceps take the right course; and sometimes experience a little difficulty in passing them through the prostate, but never made a mistake in this. However many times it may be necessary to reintroduce the forceps or other instruments into the bladder, my finger invariably precedes, guiding them, and assuring me that they take the right course.

Although I thus use the finger to ascertain the progress which is being made with the knife, and to dilate or tear the prostate, I am very careful not to dilate or otherwise interfere with the tissues *external* to the prostate and superficial to it. This is an important point with which I was first impressed when operating upon a dead subject—a lad. I found that by moving the finger about, though unconscious of having done so freely, I had created a cavity between the bladder and the rectum which I at first thought was the bladder. I have seen the same thing happen in the living subject more than once, and to experienced operators, and I believe it to be no unfrequent occurrence in children. The prostate in them is small, and there is often a difficulty in passing the finger through the incision, along the staff, into the bladder. The cellular tissue external to the prostate is soft and distensible; and in the movements of the finger attempting to find its way into the bladder an artificial cavity is produced which is mistaken for the bladder, and in which the forceps are probably thrust before the mistake is discovered. It is needless to say what frightful mischief may now ensue from the opening and closing and other movements of the forceps, in search of the stone, in rough or nervous hands. The records of lithotomy—and we need not search beyond the recent records—supply numerous disasters which may be thus accounted for. I am especially anxious to call attention to this source of blunder in the operation because I am not aware that it has been clearly pointed out by others. I was accidentally apprised of it myself in the way I have mentioned. In operating on children I think it most important to use the finger very cautiously in the deep part of the wound, to beware of letting it roam about in the soft loose tissue there, and to compel it sedulously to follow the knife along the staff till it is positively within the bladder. The staff should not be withdrawn or the forceps taken till the finger is in contact with the stone; and the forceps should be guided along the finger into the bladder, so that the operator has the assurance of the blades being in the bladder before he attempts to seize the stone with them.

It appears to me that the difficulties of the cutting part of lithotomy are rather understated, and that it is a mistake to say that the difficulty begins when the cutting is over. In children, certainly, there is apt to be difficulty in opening the bladder. Indeed, my own experience and my observation of others indicate that this is by no means the least difficult part of the operation. The distance of the bladder from the perineal surface in young subjects, the indistinctness of the structures, the prostate especially being small and not easily recognised by the finger, and the soft yielding quality of the cellular tissue, combine to cause trouble in this stage. I have seen the difficulty with the gorget as well as with the knife, and remember one of the most experienced of operators passing the gorget and forceps behind the bladder into the rectum, and astonished to find fæces instead of a stone. The bladder had not been opened. Attempts were made to re-introduce the staff, but it ran into the wound. After much delay, another surgeon succeeded in inserting a director through the wound into the bladder, incised the prostate, and finished the operation. The patient, a lad, recovered quickly and completely.

The old fashioned forceps, with blades sufficiently wide and curved in their length to hold the stone well, are, I think, the best: they are simple and easy to manipulate, and the extent to which the blades are separated is at once known by observing the handles. They should be selected rather large in proportion to the size of the stone, because the stone is more easily seized than with a smaller pair, and more easily extracted when it is well covered by the blades. It is a very common mistake to select them too small. Often, when I have experienced a difficulty in grasping the stone, and wondered what was the cause of the difficulty, I have at once overcome it by using a larger pair. I do not cover the blades with linen or any other material, because that renders it less easy to know when the stone is touched by the forceps, or when it is being included between the blades or slipping from them.

I often use the scoop, especially in children, and where the stone is small. It is a safe instrument, being easily passed along the finger into the bladder and kept under the cognisance of the finger during the whole period of its use. The handle should be flat and rough; and there is a convenience, in some cases, in having the bowl inclined a little upon the blade, as it then sweeps over a greater surface, during the rotation of the handle, than when it is continued quite straight. It was by means of a sharply curved and strong scoop, after failing with the forceps, that I extracted the largest stone I have seen removed: it weighed six ounces and a half. The patient recovered without a bad symptom.

The finger in the bladder has ascertained the position, size, and shape of the stone, and adjusted it favourably; and the forceps selected accordingly, are directed towards the spot. In searching with them for the stone, and opening and closing the blades, and in extracting the stone, there should be no hurry and violence, but cool, quiet gentleness. Celerity is of very little importance in comparison with safety, and a few minutes more or less is of slight consequence. The cautious use of the forceps searching for the stone, or their repeated introduction, does little or no harm; whereas the hurried attempt to emulate those surgeons who speak of catching the stone as it is carried forwards by the gush of urine when the forceps first enter the bladder, may lead to the needless laceration of parts, or the thrusting of the forceps between the bladder and the rectum. When the forceps have reached the situation of the stone indicated by the finger, if they be gradually opened wide, and at the same time pressed gently onwards in the direction of the stone, and then closed, the calculus will commonly be found between the blades Often, however, there is a good deal of trouble in seizing it. This is acknowledged by all operators, so that no one need be astonished or disturbed at it.

Sometimes the parts offer a good deal of resistance to the extraction, especially if the prostate be large and the stone also large; and considerable force may be required. I employ it cautiously and take time; for by gradually-applied force not much injury is done beyond the stretching and tearing absolutely necessary to admit the passage of the stone. The lesion of continuity by this plan is at the minimum. Whereas cutting, especially cutting the tissues stretched upon the stone with a bistoury passed between them and it, as recommended by some, may do much harm, and divide more than is requisite. When it is necessary thus again to resort to the knife, I press the forceps, holding the stone, back into the bladder, insert my finger along the handles, and guide the scalpel or straight blunt-pointed bistoury upon the finger.—*Lancet, April* 23, 1864, *p.* 460.

50.—LITHOTRITY WITHOUT INJECTIONS.

By Henry Thompson, Esq., Surgeon to University College Hospital.

Hitherto all operators have agreed in recommending that the urine should be first withdrawn, and that from four to six ounces of warm water should be injected into the bladder before introducing the lithotrite for the purpose of crushing the stone. It has been assumed that the presence of at least that quantity is essential to protect the walls of the organ from injury when the lithotrite is opened and closed. And further, it has been considered desirable that the fluid should be present in *known* quantity. Hence lithotrity has often been regarded as inadmissible in a case where the bladder has been so irritable as to contain only an ounce or two of urine; and lithotomy, or a prolonged course of sedatives, baths, and injections, usually ending in disappointment, has been resorted to with the view of enabling the bladder to retain the orthodox "four to six ounces."

I am quite sure that this quantity is unnecessary. Of late I have been content with two or three ounces, and, taking proper precautions, have crushed with the best results in an ounce of fluid. Neither does it appear necessary to know the exact quantity before commencing; for on first opening the lithotrite in the bladder, which the operator does very gently as feeling his way, the amount of space available for his manipulations is at once manifest. Moreover space in the bladder does not necessarily correspond with the presence of some fixed quantity of water therein. In some conditions of the bladder—or, to speak more accurately perhaps, in some bladders—two or three ounces afford as good a working area as five or six ounces in others.

It may be said—What is gained by the omission to inject? A very considerable advantage. It appeared to me very early in my experience of lithotrity, practised by others as well as by myself, that most of the untoward occurrences met with arise either from too much or too rough manipulation, and that any step towards the improvement of the operation must for the most part be made by diminishing the amount of instrumental contact with the bladder and urethra. Hence, instead of introducing a catheter to draw off the patient's urine, and applying a syringe to inject a known quantity of water, I asked the patient to retain the urine for a little less than his accustomed period before the sitting; that is, if naturally he was able to retain his urine for about an hour, he was requested to pass it forty minutes before the time of the visit. The lithotrite was then at once introduced, and the crushing proceeded with.

It is certainly undesirable to operate when the patient is urgently wishing to pass urine; hence it is as well to commence rather too soon than too long after the last act of micturition. In this manner the operator deals with a bladder not yet aroused to action, as it is sure to be when a catheter has been introduced, and when, moreover, the viscus has been unnaturally distended: for however slowly and gently a syringeful of liquid is thrown into the bladder, such injection is more irritating than the oozing in of the natural secretion by the ureters. An entire "sitting," then, consists in introducing the lithotrite; in crushing the calculus five or six or a smaller number of times, for which two or three minutes is a sufficient period; and in withdrawing the instrument.

Such may be regarded as the rule of practice. But when the bladder is much atonied, its coats being deficient in tone, and a large portion of urine remains behind after each act of micturition, it is mostly advantageous to empty the bladder, and inject a few ounces of cool water. The stimulus of water at 60° or 70° Fahr. sometimes gives tone for a time to the muscular coats, and so aids in producing a better formed cavity for operating in than a capacious, atonied, and flaccid bladder presents.

Next, in reference to injections made subsequently to the crushing of the stone, little or nothing appears to be gained by their employment. Three or four rapid injections through a large evacuating catheter generally cause more distress, and are certainly calculated to do more mischief, than the operation of crushing. Besides it is not the best time to make them in relation to the object of their application. If used at all, it should be after nature has been allowed a period of three or four days at least in which to expel the *débris*. It is a remarkable power that which the urinary apparatus possesses of expelling foreign bodies, not only from the cavity of the bladder, but from the innermost termination of the organs in the kidney, and it appears perhaps to be scarcely enough relied on by some operators. It is a most happy provision for the safety of the individual, and, after all, relieves humanity of an infinitely greater number of stones than the surgeon does. He only comes in to remedy the exceptional failures of nature. I like to feel how efficient an ally there is for the lithotritist in this said power, and to leave the expulsion of the *débris*, when properly pulverised, very much to those admirably adjusted arrangements existing for the purpose; and my experience of their capability in this respect is considerable and satisfactory. Only, when it fails, we must, as before, step in to aid nature again, and promptly.

On referring to my case book, I find, in relation to the first question, that I have crushed upwards of a hundred times without using a preliminary injection; and, in relation to the second question, that I have completed successfully eleven cases of lithotrity, most of them recent, without once using the evacuating sound. The *débris* have been easily and entirely expelled by the natural powers of the patient.—*Lancet, Feb.* 20, 1864, *p.* 209.

51—LITHOTRITY WITHOUT INJECTIONS.

By George Pollock, Esq., Surgeon to St. George's Hospital.

[Mr. Pollock, in a paper elicited by the preceding article of Mr. Thompson's, says the latter seems to have considered that "a considerable improvement on the usual practice," which is, in reality, an old method of practice.]

In the wards of St. George's Hospital, and in private, I have for some time past discontinued the use of injections prior to the operation of lithotrity. In a case of stone in the bladder of an adult, and on whom it is decided to perform the operation of lithotrity, to inject the bladder previous to the introduction of the lithotrite has long appeared to me, not only a superfluous interference, but an unnecessary prolongation of operative manipulation. In a sensitive individual, or in the case of an irritable bladder, an injection appears rather to increase than diminish annoyance, and mechanically affords no aid; for a stone, readily detected on the introduction of a catheter for the purpose of injecting the bladder, often less readily falls into the gripe of the lithotrite than if no injection be employed.

It has never been satisfactorily proved to me that preliminary injection in cases of lithotrity is either requisite or beneficial in any degree. My experience has entirely gone to prove that, as a rule, there is no occasion for it. Why should it be otherwise? Whatever quantity of urine the bladder is capable of retaining is all-sufficient for the purposes of the operator and for the successful accomplishment of the operation. If a bladder will not hold more than three ounces of urine, that quantity will suffice to enable the surgeon to crush the stone, without difficulty and without danger. If the bladder will not hold more than three ounces of urine, rarely will that viscus hold a greater quantity of tepid water; and, other things being equal, there is no benefit in tepid water over urine as a medium in which to crush a calculus; so that it does not appear that there is any advantage in the use of preliminary injection, even in the case of an irritable bladder. If the bladder will not hold more than a small quantity of urine, it will not probably hold more tepid water; and so, to the operator, the evil is equally balanced between the capacity of holding the one or the other.

Therefore, if the urine is not drawn off prior to the introduction of the lithotrite, there is *pro tanto* the less irritation to be endured by the patient.

During the past few years several successful cases of lithotrity in the female have occurred in St. George's Hospital; and many equally successful cases have been recorded in the journals, from other hospitals. But the testimony which my experience enables me to furnish in such operations bears, I think, strongly in the balance against the use of injections prior to lithotrity. In the female, on the introduction of the lithotrite into the bladder, the greater portion of the urine immediately escapes. I doubt whether three ounces of urine, or water, were ever retained in any such instance during the act of crushing a stone; it is more probable that in most, if not in all cases, the bladder was almost empty. But in all, calculi have invariably been readily and successfully crushed, without local irritation or injury.

If we may draw any analogy based on justifiable reasoning, it is but fair to infer that if a stone be successfully crushed in a female bladder under such conditions as have been stated, there is not much reason to apprehend danger under circumstances somewhat similar, but usually more favourable, in the male; for in the latter the bladder can rarely be so entirely emptied on the mere introduction of the lithotrite.

There is another remark in the observations of Mr. Thompson which I am sure he will thank me for drawing his attention to and correcting. He states that "hitherto *all operators* have agreed in recommending that the urine should first be withdrawn;" and subsequently that from four to six ounces of water should be injected into the bladder, before the introduction of the lithotrite for the purpose of crushing a stone. It is to be regretted that care was not taken to ascertain the practice of the many experienced surgeons of the metropolitan hospitals in this respect, before this statement was made; for while my own practice disproves its accuracy, I cannot but think that men of more matured experience than either of us, and with equal if not superior powers of discrimination, may have arrived at results similar to our own.

My own experience of lithotrity—first gained in the wards of St. George's Hospital, under the earliest instruction of our great teacher Brodie, and later under the practice of those who worthily followed him, and now matured by not a few years of personal observation—has taught me, as others must also have learned, that there are numerous cases in which patients afflicted with stone in the bladder cannot retain six or even four ounces of urine at a time; much less can the introduction of that specific quantity of tepid water be borne, at any rate not until the stone has been once crushed.

Such are the results of my personal experience, and such are the principles that have been inculcated on those who have associated with me in our daily work in the wards of St. George's. I see no reason to anticipate that the experience of others, with equal if not larger fields for observation, will materially differ from mine or contravene the statements herein made.

The treatment usually pursued by myself in cases of lithotrity is the very simplest that can be adopted so as to obtain the object in view. The patient is requested to retain his urine for a few hours, if possible, prior to the introduction of the lithotrite. At the time of the operation he lies flat on his back, sufficiently low in his bed to ensure the entire trunk being on one plane, that the shoulders be not on a higher level than the pelvis. The pelvis is not raised in any degree above the rest of the body. It is rare that this rule need be departed from. The lithotrite is introduced without any preliminary use of a catheter; and as soon as the stone is crushed as much as may be considered advisable, the instrument is withdrawn, and the bladder left without further molestation for that day. A small dose of laudanum is usually given in warm brandy-and-water immediately after the operation, with a view to relieve discomfort and ward off a rigor.

Mr. Thompson states that "little or nothing appears to be gained" by the employment of injections after the crushing of a calculus. If, however, injections are to be recommended or employed, it is certainly *after the operation* that they appear occasionally and not unfrequently to ensure comfort, and to be attended by advantage.

There are cases in which a catheter must be employed to draw off the urine after a stone has been crushed. A patient is occasionally unable to pass his urine without the aid of an instrument. A patient at present under my care has passed the entire *débris* of a considerable mass of stone through a catheter, for no urine can be passed otherwise. In such cases the urine is generally alkaline, loaded with ropy mucus, often offensive, and the bladder extremely irritable. Under such circumstances, the comfort and relief from tepid water injections, occasionally used, are so marked that patients have requested me to have recourse to them, when once their efficacy has been experienced, to relieve discomfort and to get rid of fragments. I only speak of what has occurred to me, but there is no reason why my experience should be exceptional in its results.

The catheter through which the bladder should be injected subsequent to crushing a stone had better be of the largest size the urethra will admit with comfort, and should be furnished with a proportionately capacious and longitudinal eye on the anterior face of the curve. With a large catheter, and through a correspondingly large eye, fragments of stone will more rapidly pass away than if left to the mere chances of natural escape.

As far as my observations extend, the maximum size of calculi which readily pass through the urethra, without mechanical aid (either of catheter or other instrument), is usually restricted by the weight of four grains, although occasionally we observe larger ones passed. With the aid of a large-sized catheter (No. 11 or 12), fragments of a heavier weight may be removed without much difficulty or pain; but in this respect much more depends on the shape than on the size of a calculus. It must, however, be borne in mind as most important, that it is not so much the size of the fragments as the amount in weight of the detritus which a catheter and occasional injections enable us rapidly and readily to remove after a stone is crushed.

All surgeons will agree with the decision, that it is far preferable to crush a stone thoroughly, so as to allow the fragments to escape in small pieces, than to attempt to remove large fragments through the urethra by any mechanical means.

Mr. Thompson applies the term "sitting" to each crushing of a stone in the bladder. On consideration he must see that this term is misapplied, and is apt to mislead. We give an artist a "sitting" who takes our portrait. In surgery accuracy of language in writing is as essential in its way as accuracy of diagnosis is in practice. Under the operation of lithotrity the patient is on his back; the operator stands by his side. Let us apply words to their right meanings; the literature of medicine will lose nothing by the restriction.

Lithotrity is, of all operations in surgery, one of the most simple, one of the safest, and one of the most successful that we have to undertake. Upon its simplicity depends its little danger to the patient; and, given a healthy state of the kidneys, we may almost pronounce the operation in such a case unattended by risk. A light touch, and a very moderate amount of mechanical skill, are the chief requisites for its successful performance. "If prudently and carefully performed, with a due attention to minute circumstances, (the operation) is liable to a smaller objection than almost any other of the capital operations of surgery" (Brodie.) And to prove the truth of the words of the eminent surgeon, who has left on record the result of his vast experience, it is only necessary to recall to the recollection of the reader, that in 115 cases operated on by Sir B. Brodie, in five cases alone fatal results were attributed to the effects of the operation itself.—*Lancet, March* 12, 1864, *p.* 295.

52.—LITHOTRITY WITHOUT INJECTIONS.

By Thomas P. Teale, Esq., F.R.S., Leeds.

[Mr. Teale, in a letter to the Lancet on this subject, says :]

I can, with confidence, add my testimony to the value of the practice advocated by Mr. H. Thompson, of performing lithotrity without the preliminary injection of the bladder. This practice is, however, more extensively adopted than Mr. Thompson seems to be aware of Mr. Pollock has shown that in his own practice he has for some time discontinued the use of injections before introducing the lithotrite.

In a paper which I published in a contemporary journal of December 10th, 1859, the following passage occurs:—"In the fifteen cases (of stone treated by lithotrity) in Table 3, the number of 'sittings' in fourteen is recorded, the smallest number being two, the largest, twenty-two, and the average number in each case about six. In all these operations the utmost gentleness of manipulation was practised, and this was much promoted by using a screw lithotrite, as small and light as was consistent with sufficient strength. The patient was generally laid on a couch, the pelvis being raised by pillows. Before the operation the patient was desired to allow the bladder to become as fully distended with urine as it could easily bear. By observing this precaution I have very rarely had to inject the bladder with water. The irritation caused by the introduction of a second instrument was thus avoided, and I have imagined that the bladder is more tolerant of the presence of urine than of the foreign fluid water. For this practical suggestion I am indebted to the writings of Mr. Syme."

I have, moreover, in all cases of stone which I have treated by lithotrity, invariably abstained from washing out the bladder to bring away the fragments, and have allowed the bladder, unaided, to expel them. I am well aware that cases may occur in which the contractile power of the bladder is so deficient as to render necessary the employment of the catheter and syringe. I have long discontinued the use of the pillow under the pelvis as being unnecessary.

It is desirable, as stated by Mr. Pollock, that the inaccurate word "sittings," as applied to the several operations, should be discontinued—*Lancet, March* 26, 1864, *p.* 367.

53.—ON THE MEDIAN OPERATION OF LITHOTOMY.

By Henry Thompson, Esq., Surgeon to University College Hospital.

[In some remarks upon a case of lithotomy, Mr. Thompson called attention to the value of the anklets designed and used by Mr. Prichard of Bristol, for securing the patient in the lithotomy position.]

He then stated that he preferred the median operation to the lateral, even for stones of the size which had been seen in the theatre; and, also, why he selected this plan in preference to lithotrity for these particular cases. It was quite possible to crush such stones, but he did not think it advisable. Lithotrity was a very favourite operation with him, as was well known; and he regarded it as most admirably adapted, especially in elderly and debilitated patients, for small and medium-sized stones, such as those from two patients recently discharged cured from the hospital. But when we had to deal with hard and compact stones, weighing an ounce and upwards, he thought that median lithotomy was on the whole a safer and better proceeding, reserving the lateral operation for stones of the largest size. There was one thing needful, however, in his opinion, in performing it, and it was a matter of the highest importance if success was to be ensured—viz., to take pains to secure the stone in good position in the forceps before withdrawing it, and to give time and patience to the extraction. The patient's life often depended more upon this single circumstance than on any other. There must be no attempt to drag it out in a hurry. Slow and steady traction, never to be hastened unduly by a single second for the sake of the bystanders, would enable you to bring safely to light a stone of very considerable size through the simple incisions of the median operation. With regard to those incisions, he had for some time preferred the mode of making them which they had seen practised—viz., the dissection downwards from the surface of the perineum to the staff, and on to the prostate, instead of making a direct puncture and cutting upwards and outwards, after the manner adopted by Mr. Allarton, to whose perseverance and sagacity we undoubtedly owed the recent adoption of median lithotomy. In the method which he (Mr. Thompson) adopted, the palmar surface of the left index-finger should be closely applied to the upper wall of the rectum while making the incisions. This enabled the operator easily and accurately to judge of the proximity of the blade to the bowel, and to make a freer and more funnel-shaped opening for the removal of the stone. Such an opening was more conformed to the relations of the adjacent viscera, and enabled us to extract larger stones with less difficulty.—*Lancet, Dec.* 5, 1863, *p.* 650.

54.—A CASE WHERE A PORTION OF TOBACCO-PIPE WAS BROKEN INTO THE BLADDER.

By H. Smith, Esq., Assistant Surgeon to King's College Hospital.

J. H., aged nineteen, was sent to my house on Saturday, Jan. 16th, by Dr. Riding, with the following story:—Three months previous, having a difficulty in micturition, he conceived the idea of passing a long clay pipe down his urethra into his bladder. He was so successful in his exploit that he repeated it, ten days before I saw him, on a similar emergency occurring, but on withdrawing the pipe he found that it was shorter by at least two inches than it ought to be. Relying, however, upon the efforts of nature to expel the body, he consulted no one, although he had great pain and irritability of the bladder, which went on increasing until his parents, hearing his groans whilst he was passing urine, made him confess the accident. He was immediately taken to Dr. Riding, who sent him to me.

On introducing a sound, I at once struck the pipe, lying on the right side of his bladder. The urethra was fortunately a capacious one, free from stricture, and not at all sensitive. I sent him home, told him to go to bed, and retain his urine for three hours before my arrival, and on the same afternoon I proceeded to operate. I at first had some hopes of being able to extract the entire portion of pipe by means of a very fine lithotrite, but, either owing to my own awkwardness or to the abrupt curve of the instrument, I had so much difficulty in introducing it that I changed my tactics, passed in an ordinary-sized lithotrite, and, readily catching the foreign body, broke it in two pieces. I then seized one of the halves—the waxed end, as it proved to be, and broke it up. I then washed the bladder well out with warm water, using a catheter with very large eyes, but I got nothing away. At ten p.m. I called on the patient, and found that he had passed about half of the waxed end of the pipe, and he was very comfortable. On calling the following morning, to my great astonishment I found that the patient had passed in its entirety the half unbroken portion of pipe, measuring exactly one inch and an eighth, and numerous fragments besides. On placing them all together, it was evident that the whole two inches had come away. I washed the bladder well out on that and the following day, removing a quantity of powder and minute fragments of clay, and on the next day but one the patient walked a long distance to my house, free from every symptom.

This case is an interesting addition to the two cases recently published in the Lancet, where in the first instance Mr. Fergusson removed an entire bougie from the bladder by means of a lithotrite, and in the second Mr. Henry Thompson was equally successful in extracting a hair-pin, and, together with them, it goes to prove what may be done with the lithotrite in cases of foreign bodies in the bladder. It is doubtful whether I could have succeeded in extracting two inches of a rigid body from the bladder along the urethral canal with safety had I introduced a proper instrument. However, the result of the treatment I adopted, if not so brilliant a *coup de main* as in the cases referred to, was equally satisfactory. There is one reflection which this case suggests in reference to perhaps the most important point connected with lithotrity—viz., as to the treatment of fragments. The speedy and spontaneous expulsion of the foreign body after it has been broken up conveys the hint that we should be content with simply breaking up a stone, and leaving Nature to do the rest. At the same time, however, it must be borne in mind that in this case the bladder was perfectly healthy; whereas in cases of stone we often meet with a bladder more or less diseased, sometimes partly or entirely paralysed, and then we appreciate the value of that treatment which consists in extracting the greater portion or the whole of the fragments by means of a small and well-constructed lithotrite.—*Lancet, March* 26, 1864, *p.* 350.

DISEASES OF THE SKIN.

55.—ON THE PHYTOPATHOLOGY OF THE SKIN, AND NOSOPHYTODERMATA, THE SO-CALLED PARASITIC AFFECTIONS OF THE SKIN.

By Erasmus Wilson, Esq., F.R.S.

No better term than Nosophyta has been suggested by any of the phytopathologists who have succeeded Gruby; it expresses the bare scientific fact of the association of a plant-like organism with certain diseases; it commits those who make use of it to no theory; and therefore we adopt it as the title of our present essay.

The Nosophytodermata are five in number, namely:—Favus, *vel* Tinea favosa; Trichosis, *vel* Porrigo tonsurans; Mentagra, *vel* Sycosis; Alopecia areata, *vel* Porrigo decalvans; Chloasma, *vel* Pityriasis versicolor. These diseases form a natural group, which is distinguished by a morbid alteration of the epidermis and hairs, rendering them friable and brittle, changing their appearance and colour, and causing their exfoliation and fall.

The epidermis loses its smoothness, the hairs lose their polish; the former becomes spongy and furfuraceous, the latter become faded, discoloured, and shrivelled, and break off close to the skin, suggesting the idea of being cropped by artificial means; hence the terms, tonsurans, tondens, tonsoria, &c., applied to one of these diseases.

Closer observation detects certain differences between them—for example, favus and mentagra affect chiefly the hair-follicles; trichosis and area chiefly the hairs; and chloasma chiefly the epidermis. Favus has the appearance of a yellow flat pustule, often of considerable breadth (two or three lines), surrounding the mouth of the hair-follicle and hair; trichosis is known by the broken state of the hair, which gives an aspect of bareness or seeming baldness to the diseased parts; mentagra is a prominent pustule with a hard and often elevated base surrounding the mouth of the hair-follicle and hair, and situated, as its name implies, on the chin, or on the face; porrigo decalvans or area is a positive baldness of the affected part, often associated with loss of pigment and atrophy of the skin; while chloasma is known by its situation on the trunk of the body, by an alteration of the pigment of the skin—which has gained for it its name—and by a loose and spongy state of the epidermis, which exfoliates from time to time in the form of a branny or furfuraceous desquamation, pityriasis versicolor, or, as Hardy very appropriately suggests, pityriasis lutea.

The general characters of these affections, therefore, so far as *resemblance* is concerned, are, the seat of the disease—namely, the hair-follicles, the hairs, and the epidermis,—and the consequences of the disease—namely, the disintegration and destruction of the tissues involved, the epithelium of the follicles, the epidermis, and the hairs. The *differences* of a general nature are the limitation in chief of the disease to the follicles, to the hairs, to the epidermis; the intensity of the special affection, which is greatest in favus and least in chloasma; and the complication of the special affection by common inflammation, as occurs in mentagra.

[Mr. Wilson then describes the nature and causes of these diseases in his usual masterly way, and lastly, gives us his treatment.]

Treatment.—The treatment of the phytodermata offers three indications:—The *first*, to destroy the parasitic fungus; the *second*, to subdue the inflammation which accompanies the growth of the fungus; the *third*, to relieve the secondary or constitutional eruptions which complicate the disease in its advanced stage. The remedies suited to the first of these indications are termed by the French phytopathologists "parasiticides," and with especial reference to the vegetable fungus "phyticides." Bazin enumerates as the chief of the phyticides, the oleum picis juniperi (oleum juniperi pyrolignicum, huile de cade), the bichloride of mercury, and turpeth mineral (hydrargyri subsulphas flavus). But another, and the most important of the phyticides, according to him, is depilation or avulsion of the diseased hairs. The second indication is met by antiphlogistic remedies of the emollient or moderately stimulant class, such as the cold starch poultice, fomentations, cold ablutions, saturnine lotions, the benzoated oxide of zinc ointment, and the ointments of nitric oxide of mercury, or the ammonio-chloride of mercury considerably diluted. While the third indication, the secondary complications of the disease, is to be treated by similar means.

Bazin describes his methed of treatment of the tineæ as follows:—The head is to be thoroughly cleansed; crusts are to be removed, and the hair cropped close to the scalp; then the surface is to be painted over with the oleum picis juniperi, which, as he states, soothes the skin, renders it less sensitive, loosens the bulbs of the hair, and at the same time shrivels and destroys the fungus. The next day, the head is to be washed with soap, and depilation is commenced; that is to say, every hair is to be removed over the whole extent of the diseased surface. The accomplishment of depilation may possibly require four or six sittings, for the process is too painful to be borne for a long period at a time, and may require to be repeated twice or three times, or even more frequently, before the cure is effected. The operator seats himself in a chair, and places the patient on a cushion at his feet; he rests the patient's head on his knees, and then, with a pair of tweezers, patiently removes the hairs, one by one, or in bundles of two or three, when they can be fairly seized by his instrument. Having pulled out the hairs over a small extent of surface, he brushes it over with a solution of the bichloride of mercury to destroy the fungus, both on the surface and in the patent follicles; and then goes to work again in the same manner, from time to time resting, to moisten the skin with the solution, until he has proceeded as far as the patient can bear.

Four or five hours after this operation, the head is to be thoroughly anointed with a parasiticidal ointment, of which the two following are the best, and Bazin gives a preference to the first:

℞. Hydrargyri subsulphatis flavi, ʒss.; olei amygdalæ dulcis, glycerinæ destillatæ, āā ʒij.; adipis purificati, ℥ij.; M. Fiat unguentum phyticidum.

℞. Olei juniperi pyrolignici, ʒij.; adipis purificati, ℥iiss.; M. Fiat unguentum phyticidum.

On the following day, the washing, the depilation, the painting with the bichloride of mercury lotion, and the anointing with the phyticide pomatum are to be repeated as before, and continued daily, until every hair growing on the diseased portion of the skin, and for a short distance beyond, has been removed. This process is painful, but not according to Bazin, so painful as the avulsion of healthy hairs; and the pain may be much reduced by drawing gently but forcibly on the hairs in the direction of their implantation in the skin.

When depilation is complete, the head is to be painted over, night and morning, with the solution of the bichloride of mercury, for three days; and after the third day, is to be anointed, night and morning, with the yellow subsulphate of mercury ointment. Bazin does not mention the strength of the sublimate solution which he employs, but the solution used by Hardy contains one grain of the bichloride to an ounce, and we apprehend that if it were stronger it would produce inflammation and more pain than the patient could bear. The success of the process cannot be ascertained until the hair begins to grow again; and then, if it be unhealthy in its aspect, the operation must be repeated, if necessary, to the third or fourth time.

Hardy's treatment of tinea differs from that of Bazin only in this—that he finds no advantage in the huile de cade, and therefore omits its use; he prescribes the sublimate lotion after depilation, for eight days instead of three; and in the latter part of the treatment, sulphur ointment, containing half a drachm of sulphur to the ounce, instead of the turpeth ointment. We may in this place, also remind our readers of the sulphurous acid lotion of Dr. Jenner, and the pentasulphide of lime lotion, recommended by Mr. Astley Price, for the destruction of the oïdium of the vine; the latter being an equally successful phyticide, or, at least, remedy, when used to the human skin in these diseases.

Treatment of Favus.—In favus the first step is to cleanse the head, to crop the hair down to the crusts, to paint with the oleum juniperi pyrolignicum; then to apply a starch poultice to soften the crusts. When the crusts are sufficiently softened, they are to be gently lifted with a comb and removed, and the oleum juniperi is to be repeated as before. On the following day depilation may be performed, as already described; and in four or five days the operation will be complete.

When the body is the seat of the eruption, and the latter is extensive, baths containing sulphur, or the bichloride of mercury, will be necessary; the patient should have them repeated several times; the crusts are to be removed after the bath, and depilation may then be commenced. The process of depilation will occupy from fifteen to twenty days, and, night and morning, the turpeth ointment should be well rubbed into the skin. In favus of the nail, the horny portion of the nail must be scraped until the fungus is laid bare, and then it should be saturated with the sublimate lotion.

After the operation of depilation, the redness of the diseased skin gradually diminishes for about a month; then, there is some reappearance of hyperæmia, a few pustules are seen, and the favus-cups begin to be developed; but they are smaller and more scattered than at first. Depilation is now to be repeated in the same manner and to the same extent as before. The patient is then left for another period of five or six weeks; and if any favus-cups reappear they must be removed, and depilation of the parts where they were produced is to be performed. This is only a partial depilation: and after two complete, and one partial depilation, the cure may be expected to be complete, and no. further danger of relapse anticipated. Nevertheless, it is always necessary to watch the patient for a few weeks longer.

Bazin remarks, that if the eruption exists to the extent of five-sixths of the scalp and body, the removal of every hair the patient possesses, whether unhealthy or healthy, will be necessary for the cure; that the operation must be repeated in four or six weeks; and that at the end of another month a partial depilation must be effected. Where there are not more than four or five patches situated on the scalp, depilation of the diseased part and of the sound skin for a short space around will be all that may be necessary. And where there are not more than two or three cups, they may be cured at once, and in the course of a few minutes, by the removal of the crusts and the avulsion of a few hairs.

During convalescence, extreme cleanliness is of the first importance, any formation of scurf or accumulation of sebaceous matter should be carefully washed away, and the yellow sub-sulphate of mercury ointment applied every four or five days.

Treatment of Trichosis Tonsurans.—The treatment of this form of disease, like the preceding, consists in the performance of depilation and the use of the phyticide applications. Trichosis of the body or limbs in women and children rarely progresses beyond the first period, and the bichloride of mercury lotion, together with the phyticide ointments, are all the treatment necessary. When, however, as in man, the body is hairy, and the disease is circumscribed, depilation may be practised during the first period, and followed by the plan of treatment laid down for favus; but until the disease is fairly defined, the bichloride of mercury lotion and the phyticidoe intments are the only means to be employed.

After the first period is passed—whether the disease be seated on the body or on the scalp—depilation is to be performed in the manner already described for favus. But the operation is more difficult than in favus, on account of the brittleness of the hair, and may require to be repeated five or six times before the whole of the diseased hairs are removed.

The restoration of the skin to its healthy condition is shown by the growth of natural hair; the papillation of the patches subsides, the patches return to the level of the surrounding parts, and the scalp recovers its normal colour and appearance. The healthy growth of the hair is the best evidence of the destruction of the fungus.

Treatment of Mentagra.—On the face, and especially on those parts of the face where mentagra commonly makes its appearance, avulsion of the hairs may be practised in every stage of the disease. The oleum picis juniperi should be applied in the first instance as directed for favus, and then the avulsion may be commenced at once. Whenever, says Bazin, "Tinea tonsurans has already, for some time, reached its papulo-pustular period, depilation must be performed, even if we be sure that the parasite no longer exists; it is in these cases that the disease yields as if by enchantment to our treatment; a single depilation may suffice; and on this account it is that the cure of mentagra may be said to be easy in proportion to its chronicity." The same author remarks, that much time is often lost in vain attempts to subdue the inflammation and tumefaction of the skin in mentagra before resorting to avulsion of the hairs, under the impression that the inflammatory action would be increased by the operation; but the real agent of irritation is the hairs, which acts the part of a foreign body and stimulate the inflammatory process; and the best method of dissipating it, would be to remove the foreign body, which is the cause of all the mischief. After avulsion our remedies perform wonders.

Treatment of Alopecia Areata.—The treatment of area is to be conducted on the same principle as that of the other forms of tinea—namely, the application of phyticides and depilation. Bazin remarks that the puny hairs which are found on the bald patches of area are difficult to remove on account of their weakness, and that repeated depilations are therefore necessary; he also reminds us of the necessity of carrying the depilation to a short distance beyond the limits of the patch into the region of the sound hair.

Treatment of Phytodermatous Furfuræ.—The treatment of these affections, including chloasma, is, according to Bazin, extremely simple: no internal treatment is called for; the parasitic fungi are situated superficially in the epidermis, and the phyticidic remedies already enumerated are all the treatment required; they may be used in the form of lotion, forcible friction, or baths, according to their situation or extent. The treatment commonly pursued by this author is to apply the bichloride of mercury lotion and use sulphurous baths, but he gives the preference to baths containing the hydrargyri bichloridum.

Epitheliophyta.—Carrying out his views of the parasiticidic operation of the bichloride of mercury, Bazin recommends it as a wash for the mouth in aphtha, to destroy the Oidium albicans, in preference to the biborate of soda. And upon the same principle he favours the use of the pyroligneous oil of juniper in chronic ulcers.

Observations.—The chief point of novelty in the treatment of Tinea, as advocated by Bazin, is depilation; but this recommendation is far from being new; it was practised by Plumbe, by Rayer; it is the basis of the treatment of the Mahons, and the principle of the notorious "calotte," so long, and we fear at present, in use in France. But the objects of the treatment are different; Bazin removes the hairs because they are the harbour of a parasitic vegetable fungus, which developes seeds, and consequently, as long as it remains, keeps up and extends the disease. Plumbe, Rayer, the Mahons, and the promoters of the calotte, because the hairs are supposed to act the part of foreign bodies and irritants to the inflamed skin. "In old standing cases of favus," says Rayer, "every method of treatment into which the avulsion or removal of the hair does not enter as an element, is incomplete, and unworthy of being entitled curative."

The depilation recommended by Plumbe differed from that of Bazin in being partial; the same objection may be made to that of the Mahons, which operates only upon the loosened hairs; while the calotte is an instrument of the grossest barbarity, tearing out alike sound and diseased hairs, and in a manner the most painful that could be contrived. Bazin's method is painful enough, and to many persons would be insupportable, but every care is taken to make it as gentle as possible; the skin is rendered less sensitive by the previous application of the juniper tar, and the hairs are withdrawn artistically, no doubt in the direction of the *set* of the shaft, with moderate and steady traction, and to a limited extent at each repetition of the operation. With reference to the rest of the treatment, it differs in nothing from that which has been in common use in this country for a number of years—namely, washings with soft soap, combing and brushing the hair, and the use of mercurial pomatums, of which the nitric oxide and ammonio-chloride of mercury are examples, and tar and sulphur ointments, which in public institutions are highly popular.

With the exception of mentagra, we believe depilation to be wholly unnecessary in these diseases; we believe that they may be successfully cured, as they have been heretofore, without depilation. We never find any overwhelming difficulties in the treatment of these complaints; they are tedious, but not remarkably so when properly managed, and we decidedly declare against depilation. Depilation may be, and no doubt is, a great boon in the treatment of tinea in France, but it is worthless in England, where more care is bestowed upon the constitutional treatment of cutaneous diseases.

And what, it may be asked, is the treatment on which we rely for the cure of these diseases? We will state it briefly:—We remove crusts and furfuræ by thorough washing with the juniper tar or petroleum soap, or with a liniment consisting of equal parts of soft-soap, juniper tar, and alcohol. We repeat the washing daily with tepid or cold water, with the double object of cleansing the diseased parts and giving a moderate stimulus to the skin of the head; we comb the head well, brush it if not too sensitive, and night and morning we rub into the entire scalp, but most into the patches, an ointment consisting of one part of the nitric oxide of mercury ointment, diluted with three parts of fresh lard. In milder cases we wash the head less frequently, but we continue the combing and brushing and anointing steadily. A failure of cure is an event that we cannot anticipate and rarely see.

But we do something more: we administer mild tonics; we prescribe a generous diet; we treat symptoms; and, in a case of favus—a disease of the scrofulous diathesis—we have recourse to cod-liver oil and the iodide of iron. Moreover, in certain cases, where no indications of general debility exist, where the nutritive functions seem principally at fault, we administer arsenic, with the most brilliant success. Favus and trichosis capitis are diseases of children and adolescents, and this method of treatment answers admirably with them. In trichosis corporis (Lichen annulatus solitarius et serpiginosus) we have for many years been in the habit of prescribing a lotion, or ointment, of the bichloride of mercury; and in chloasma, or pityriasis versicolor, we prescribe ablutions with the juniper-tar soap, sponging with a spirituous solution of the bichloride of mercury in almond emulsion; or frictions at night with one of the diluted mercurial ointments already mentioned, and ablution in the morning with the juniper-tar soap. In trichosis barbæ, or mentagra, we are ready to admit the value and importance of avulsion of the hairs; not, however, as a means of removing a supposed parasite, nor of an irritant foreign body, but as a healthful stimulant to a part in a state of chronic and irritable inflammation.

And this brings us to the question of the use of depilation: that it disposes to the cure of the local disease there can be no doubt. We cannot disbelieve the statements of Bazin that he succeeds in curing these diseases through the agency of depilation; but we do and must disbelieve that the removal of the fungus is the proper explanation of cure. We see in depilation a stimulant, and a valuable stimulant—a stimulant that we ourselves commonly use when we seek to make a black hair grow up in place of one that is white—a treatment that for this purpose we have been in the habit of using for years, and with remarkable success; so that we have been led to regard avulsion as one of our best, indeed our only reliable remedy for producing *deep stimulation of the cutaneous tissues*, for setting up a new action in the papillæ of the hair; as the only remedy, in fact, by which we can reach successfully the fundus of the hair-follicle. And this, in our belief, is the "methodus medendi" of depilation and avulsion in the phytodermic diseases. It sets up a new action, a healthful inflammatory process in place of a morbid inflammatory process—a process whose natural course is to end in resolution, instead of one whose nature is perpetuity. For morbid cell-genesis it establishes healthy and plastic inflammation and sometimes healthy suppuration, when, as Bazin declares, the fungus is starved or drowned. Even Bazin has recourse to avulsion as a stimulant in old cases of mentagra, wherein the hair acts the part of a foreign body, of a thorn, in the skin, and so keeps up irritation.

Hardy makes the following very practical observation in reference to the treatment of the Phytodermata:—"It will not be sufficient to destroy the parasitic fungi only; we must modify the soil so as to render it inapt to the reception of new germs, and unfavourable to their nutrition. We must give tone to the constitution by means of tonics and bitters, prescribe good food and closer attention to cleanliness, and cut the hair short to admit of the action of the air upon the scalp."—*British and Foreign Medico-Chirurgical Review, Jan.* 1864, *p.* 199.

56.—ON PITYRIASIS.

By George Nayler, Esq., Pathological Curator of the Museum of St. Mary's Medical School.

The diagnosis of pityriasis is not difficult. It approaches much in character to psoriasis, from which it may be distinguished by the farinaceous condition offered by the cuticle, that is shed and renewed again with great rapidity. The patches of psoriasis are always more or less raised, which is not the cause in pityriasis. Some cases of eczema occurring in infancy, where the colour of the skin is hardly changed, and no vesicles detected, may be mistaken for this disease; but, on close examination, the thin crusts or lamellæ are more disc-shaped, and are neither produced nor re-produced to the extent observed in dandriff. No error is likely to arise from confounding pityriasis with lichen, as the papular nature of the latter, added to its rough surface and situation, will be enough to point out the difference between them.

Treatment.—In pityriasis of the scalp the patient should be directed to have the hair cut moderately short, to use a soft brush, and to wash the head once or twice a week with the yelk of egg and warm water. At night, before retiring to rest, a liniment of this kind should be well rubbed into the roots of the hair:—Nitric oxide of mercury, ten grains; glycerine, half an ounce; cerate, one ounce: or ammonio-chloride of mercury, ten grains; cerate, one ounce. The compound iron mixture of the London Pharmacopœia may be given, or quinine with one of the mineral acids, if the health be disordered. As a rule, at the Skin Hospital arsenic is prescribed in pityriasis capitis; and it may be administered as the liquor of arsenite of potassa. If the disease be dependent on syphilis, then, as in psoriasis, the bichloride of mercury should be given with the iodide of potassium. In general pityriasis, where the skin is dry and perspiration takes places with difficulty owing to some defect in its secreting structure, great benefit will be afforded from the use of glycerine, as mentioned by Mr. Startin in his lectures, or as a bath by boiling two ounces each of glycerine and tragacanth in a pint of water, and mixing this with thirty gallons of the latter; or, again, as a lotion in this way: one grain of the bichloride, half an ounce of glycerine, and three ounces of water. Should pityriasis attack the face, which it is apt to do in young women from exposure to cold winds, the patient may use with advantage the camphor ointment of the Pharmacopœia—ten grains of camphor, ten minims of glycerine, and an ounce of cerate; or, as Dr. Jenner recommends, apply a small quantity of zinc ointment before going out in the open air, and at the same time wear a Shetland veil to protect the face. When pityriasis surrounds the mouth, as in children from constantly protruding the tongue, some olive oil or glycerine should be smeared over the part, and the child prevented from continuing the practice.

I should add, that however useful glycerine is in certain diseases of the skin, no benefit can arise from the use of the so-called glycerine soap, which, from the small amount of glycerine it necessarily contains, is utterly valueless as a remedial agent.

In the treatment of pityriasis vesicolor, our object is directed to destroy the parasite, which may be accomplished by various means. Sulphur offers the readiest and most certain mode of effecting a cure. With this view, the sulphur vapour bath may be administered once or twice a week; and I have searched in vain for any trace of the fungus three days after a single trial of the bath. While this remedy is being employed, the patient should apply some mercurial ointment to the affected part before going to bed, as, for example, the red ointment of which I have spoken in a previous paper, and either a weak nitric acid lotion during the day, or one of this form, which is much used at the Skin Hospital: a drachm of borax, half an ounce of rectified spirit, one grain of the bichloride of mercury, and eight ounces of water.—*Lancet, Jan.* 16, 1864, *p.* 63.

57.—ON BURNS.

By **Frederic C. Skey**, Esq., F.R.S., President of the Royal College of Surgeons, and Surgeon to St. Bartholomew's Hospital.

In the treatment of burns I have had some experience. Every year confirms my conviction of the soundness of the principle first proclaimed to our profession by Dr. Kentish, of Bristol, about half a century ago—a principle approved, accepted, and all but universally adopted for twenty years and upwards. That principle consisted in the application of stimulants in one or other form as early as possible after the receipt of the injury. Although less popular in 1863, it is not less sound. How many hospital surgeons have subjected these painful accidents to the test of critical inquiry and experiment? How many can truly affirm, "I have fairly tried the two methods of treatment, and I adopt the Carron-oil system?" Of the two systems of treatment which consist of medical or other agents, the one adopts the principle of stimulation, the other soothes. The idea of soothing an irritated surface is, indeed, plausible. To soothe signifies to allay, to calm. What is the agent employed for this purpose—almost universally employed? Carron oil. Ask a man the subject of a burn what amount of relief from suffering he obtains from the application of Carron oil at the expiration of any term from one hour to forty-eight. I believe it neither affords relief nor answers any useful purpose whatever. Its employment is little better than a delusion. Yet it is the almost universal agent used in the coal and iron districts, in which injuries by fire are so frequent and so fatal. The reason why it is so universally employed in these districts may be inferred from the following anecdote. Some years since, while on a visit near the iron works of Dowlais and Merthyr Tydvil, where many thousand men were engaged, I witnessed sundry cases of severe burns; and on my urging on the resident surgeon the value of the opposite principle to that which he adopted in the treatment, he replied to this effect: "If I were to attempt to invade the prejudices of these men, and insist on a change of treatment, my life would hardly be safe from their violence." This is, indeed, valuable testimony. A very common fact in every-day life displays the value of the treatment by stimulants. The smarting pain caused by a burn or scald on the hand is relieved, and, if not very severe, is almost removed, by subjecting the hand to the influence of the heat of a fire. The closer it is held—the more severe the pain caused by the undue heat, the greater is the amount of permanent relief. What is the theory of this? I cannot pretend to give a satisfactory explanation of the phenomenon, but it is not the less true. It is palpable to ordinary observation that if we stimulate a burnt region of the body, or, in other words, if we increase the pain of the part by the application of any agent—as heat, whether from fire or hot water; or if we stimulate by the agency of turpentine, as recommended by Dr. Kentish, or spirit or stimulant of any description—we relieve the affected part from its pain, and carry it forwards by a rapid stride towards recovery. The greater pain deadens the lesser, and not for the hour merely, but permanently. And what is true in principle on the smaller scale is equally true on the larger. Of stimulants, I know no agent so efficient as a solution of nitrate of silver, which I have for some years employed in the proportion of from ten to fifteen grains to an ounce of water for an adult, and from five to seven grains for a child. This solution applied freely over the burnt surface is followed by the application of cotton wool. In an hour or less the pain decreases, and ere long subsides. I could illustrate the efficacy of this treatment by an endless number of examples, but I will mention one only. Five men were severely burnt by an explosion of gas, and were brought to the hospital. One died immediately; the remaining four were badly burnt about the face, chest, and arms. The face and chest of each man was washed with a solution of ten grains of nitrate of silver; to the arms the celebrated Carron or boiled oil was applied. Twenty-four hours elapsed, and on inquiry whether the patients were suffering pain, each made the same reply: "I am easy everywhere, except in the arms and hands." The oil was removed, the solution was applied, and relief followed immediately. The solution may be profitably applied at any period, so long as the pain remains.

On the younger members of our profession I most strongly urge the value of the stimulating principle in the treatment of burns and scalds of every description, not only for the relief from suffering which it affords, but for the influence it exerts in abridging the duration of the consequences of the injury and promoting an early recovery. The only exceptions to this benefit are found in the cases of very severe injury which are attended by great destruction of the tissues of the affected parts; but even in such cases relief from pain is always afforded by the application of the solution in or about the strength I have mentioned.—*Lancet, Jan.* 2, 1864, *p.* 2.

58.—INTERNAL ADMINISTRATION OF BELLADONNA IN CASES OF SEVERE BURN.

By — Hutchinson, Esq., Surgeon to the London Hospital, &c.

Experimental physiologists have recommended belladonna for use in the treatment of burns, in the belief that it diminishes that state of the nervous functions under which reflex inflammations are likely to be orginated. They assert, on the other hand, that of all remedies opium is the one most powerful in increasing this peculiar state, and that it ought consequently to be avoided. In clinical practice, however, we believe that this opinion is wholly disregarded, and that opium is the form of anodyne most commonly resorted to in these cases. Yet it is generally suspected that the causes of death after burns are, in a majority of instances, connected with reflex inflammations, *e.g.*, ulcers of the intestine, pneumonia, &c. In a series of cases under Mr. Hutchinson's care in the London Hospital during the last six months, the belladonna treatment has been tried. In some remarks at the bedside of a patient the other day, Mr. Hutchinson stated that he considered the general results to have been fairly satisfactory. He adverted to the extreme difficulty of forming a trustworthy conclusion on such a matter, since these cases are, in their nature, never stationary, but always tend either to improvement or the reverse, and often with great rapidity. If, therefore, the remedy were commenced when the patient was very ill, it might chance to be just at the time when improvement was about to set in; and if, on the other hand, the patient got worse, it might fairly be alleged that the remedy was used too late. If, on the other hand, we should give it in cases in which, as yet, no serious symptoms had appeared, we might again be much led astray, since a great majority of burn cases do well without any special plan of medication. Mr. Hutchinson stated that the cases in which the remedy had seemed to be most useful, were those of children in whom general febrile symptoms, attended with restlessness, loss of appetite, &c., had set in without any local complications. In several of these, there could be no mistake that the feverish state had passed away quickly and very satisfactorily under the use of belladonna. In no cases had he witnessed any ill results. If the burn itself was very painful, and the patient unable to get sleep on account of the pain, then the belladonna seemed comparatively inefficacious to procure ease, and morphia was far more efficient. As a rule, no opium had been given to the cases treated by belladonna; but in a few, and those chiefly in adults, it had been found requisite to give an occasional night dose. Possibly more benefit might have been obtained had the administration of the belladonna been pushed to larger doses. The usual dose given had been a third of a grain three times a day. In speaking of the less frequent results of burns, Mr. Hutchinson mentioned a recent case in which acute inflammation of one hip joint, followed rapidly by dislocation, had occurred in a child who had been severely burnt on the arm and chest. He was in doubt whether to regard it as a reflex inflammation, or as a consequence of pyæmia.—*Medical Times and Gazette, Jan.* 1864, *p.* 10.

59.—ON THE TREATMENT OF ANTHRAX BY PRESSURE.

By Maurice H. Collis, Esq., Surgeon to the Meath Hospital.

[Anthrax is a disease of the areolar tissue, the skin only suffering secondarily.]

It is essentially an inflammation of dense fascia, in which the superficial areolar tissue is implicated, as in furuncle, and also the deep, as in phlegmonoid erysipelas. There is along with, and resulting from this threefold inflammation, a copious exudation of lymph, which agglutinates the inflamed parts into an almost homogeneous yellowish-grey mass, destroying the vessels, and of necessity stopping the circulation; hence a large and rapidly increasing core results. So peculiar is this core that Nélaton classes it among false membranes, similar to those which form on the surface of serous membranes. There is some show of truth in this notion of Nélaton's, but it is only part of the truth, and not the main part either. The mode of extension of anthrax much resembles that of phlegmonous erysipelas—sub-fascial exudation goes on, cutting off the vascular supply to fascia, and, at the same time, exudation goes on superficial to the fascia, cutting off the vascular supply to the skin. This double exudation produces two sets of physical signs; a broad elevation, equal in extent to that of the deep exudation, with livid or dusky red hue of skin, and hard œdema, extending somewhat beyond that limit, points to deep sub-fascial inflammation; while the numerous small furuncular openings, which, running into one another, discharge small quantities of healthy pus, long before the core is loosened, are evidences of more superficial inflammation. The brighter red of the skin round these openings is another sign of their being akin to simple follicular abscesses; for though not so bright as in healthy phlegmon the colour is less dull than it is in the outlying parts of the tumour. This superficial inflammation is of less extent than the deep, for the simple reason that its products get vent more quickly. The skin only has to be perforated, whereas in the other case, deep fascia and skin must both give way, and the fascia only gives way by becoming a dead slough, and by being cast off by a process of ulceration along its edges. This is a slow process, during which exudation advances, and the diseased action extends its limits.

Dublin surgeons, as a rule, have been in the habit of treating anthrax by the crucial incision of Abernethy. Some advise that the incision should reach from sound skin to sound skin. Even Mr. Ledwich, following the stern surgery of Tagart, and undeterred by the fatal cases he himself has quoted, gave in his adhesion to this practice. I have long been satisfied that this length of wound was a needless increase of risk and suffering to the patient, and that the mere extent of a crucial incision was of little consequence in comparison with its depth.

If not deep enough to allow free vent to the dead core and matter, an incision is worse than useless—it is positively mischievous, and causes the disease to spread. To be deep enough it must penetrate the core, and tap the inflammatory exudation and purulent depôt which lie beneath it, and which would not otherwise find a vent until the core had separated. When making the crucial incision it is easy to know if we have gone deep enough for our purpose. Should there be still much vitality in the integument, which we have cross-cut, the wound will gape widely, and the point of each angular flap will curl up considerably. Even in advanced cases, where the skin is much undermined and thinner, and where it has not sufficient elasticity to curl up or retract, we shall be able to know if we have gone deep enough by lifting up the angles of the flaps in a forceps and feeling if they are well loosened from the parts underneath. If they do not yield at once, and freely, we must cut deeper until we reach the subjacent muscle. The crucial incision then, if adopted, must be deep to be of use in checking the spread of the disease and facilitating the repair of the affected part.

But it may be well to inquire if an incision is always, nay, if it is often required. It is surely in accordance with the merciful progress of modern surgery that we should, at least, pause now and then in any practice which has become routine, and ask if it be never right to depart from it.

I shall not here enter upon the treatment of anthrax by the more painful plan of the potential cautery, which is wholly inapplicable in the early stages of the disease, and which cannot, at any stage, be supposed to check its spread. I would rather venture to inquire if any painful application or treatment is required, and whether we may not, in the great majority of cases, bring about a favourable issue without giving pain, and with greater rapidity than either knife or caustic can effect. Under the advice of Mr. Paget, whose calm judgment must carry weight with all intelligent surgeons, and encouraged by his example, I have not, for two years and a half, used either knife or caustic potash in anthrax; and with the results I have every reason to be satisfied. I do not say that I shall never cut an anthrax or use caustic potash to detach the sloughs—but I shall simply give examples of various forms and degrees of the disease as treated by me without either; and, I think the results authorise me to conclude that the cases in which it is necessary to resort to either are rare indeed.

The first case in which I tried the effect of a milder plan of treatment was one which most people will allow to have been a legitimate case for it. It was a large anthrax between the scapulæ, occurring in an old and feeble man. It had been left to nature, and had spread until it reached the size of a small plate; along the centre was a brownish slough four and a-half inches long and three and a-half wide. This slough was still firmly adherent, the line of demarcation having just commenced to form. Under this cutaneous slough, and raising it high above the level of the healthy skin, lay a boggy mass of dead fascia, areolar tissue, and pus, which, as yet, had no vent; but it was evident that the fascial slough was sufficiently detached to allow the subjacent pus freely to come forward beneath the skin. Hence, the anthrax had ceased to spread; yet all round the slough was the livid and hard margin. still the seat of great pain, and quite capable of carrying further, upon the least provocation, a gangrenous inflammation.

I immediately applied a large plaster of emplastrum saponis cum opii, spread on wash-leather, with a crucial incision in the centre of the plaster corresponding in situation with what would have been, under ordinary circumstances, the crucial incision in the anthrax. The next day the swelling round the slough had sensibly lessened, and had become of a healthy red; the pain had completely disappeared; some discharge appeared at the margins of the slough; this increased from day to day, and the slough separated much as it would under any other circumstances. The plaster was renewed from day to day, and the cure was completed in less than a month, which, for so large an anthrax in a feeble old man, was as much as could be expected.

This case showed that anthrax will sometimes stop spreading without the surgeon's help, even in old broken down persons. It also shows that support to the capillaries will relieve the pain of anthrax, as is ably pointed out by Mr. O'Ferrall, in the fifth volume Dublin Hospital Gazette. In that paper Mr. O'Ferrall speaks highly of the treatment of anthrax by pressure, but, as it appears to me, without venturing to carry out his principles to their full and legitimate conclusion; he does not seem to contemplate the use of pressure alone apart from the knife. His remarks, however, on the results of pressure after incision, in a case of his own, are of much importance. In that instance the anthrax had been freely divided by the crucial incision; yet, it had steadily spread, unchecked either by the general use of wine and bark, or by the knife, followed by a free use of nitrate of silver; yet, no sooner was support afforded by the firm application of soap plaster, spread on thin leather, than the disease began to diminish, and all suffering ceased. Mr. O'Ferrall's words are :—

"The effect of pressure was immediate and remarkable. The morbid sensibility which caused the patient to shrink from manipulation was at once diminished by well regulated pressure. The following day the other phenomena of the disease—hardness, swelling, and dusky redness, the results of capillary congestion with infiltration, occupying an area of one inch and a-half—had entirely disappeared. Nothing could be more evident than the utility, in this case, of the treatment of anthrax by pressure."

After trying pressure by sedative plasters, in a number of patients, confined to bed, and supplied with good diet and other comforts, I determined upon attempting to treat the disease without such aids in order to test the plan of treatment.

I selected a man of blanched aspect, prematurely old-looking, and with all the marks of poverty about him. This man had two anthraces—one on the point of the right scapula, of large size, and a smaller one in the axilla, near the scapula. The large one was about four inches by three in diameter, as regards the central boggy portion, with a livid margin, a couple of inches wide, all round. The small one, more prominent, was also more advanced, although later to appear. The core was beginning to separate in it, and it had ceased to spread. The larger one was still spreading—it was the seat of deep and burning pain, and had not commenced to open spontaneously. I strapped both with spiral strapping, covering the entire of the larger, and all but the centre of the small one. Not having soap plaster with opium, at hand, I used common adhesive plaster. In twenty-four hours the improvement was marked. The pain had lost its burning character, the swelling had decreased, the lividity of the centre part was changed for a healthier red, and the edge of the outer margin was paler. The improvement continued from day to day; a small central opening gave vent to a good deal of healthy pus; and in ten days not only was the core out completely, but the cavity from which it came was mostly healed up; and in three weeks he was well. I gave him a little bark during this period. He had no means to enable him to get good food or stimulants, and continued as an extern all the time.

From these cases, to which many others might be added, it is plainly evident that anthrax may be treated sometimes without incision. It is, I think, clear enough that we may pause over each case, and consider if it be necessary or advisable to resort to the knife. For my own part I am satisfied that, as a rule, anthrax need not be cut. I do not wish to be dogmatical, or to lay down an absolute rule that the crucial incision is to be entirely abandoned, but I am entitled to ask that it should not be held to be a *sine qua non*.

The question may be asked, how pressure acts. Mr. O'Ferrall has well answered this. He states that the capillaries in anthrax are in danger of losing their vitality under the severity of a gangrenous form of inflammation, and that the support of even pressure enables them to resist the destruction which menaces them. This is, no doubt, the explanation. In practice we find pressure answer, let the explanation be what it may.

I have observed that as soon as the parts get support, the extension of sub-fascial exudation ceases. This is the natural result of giving support round the margin of the inflamed part. The efforts of the matter to get vent are then directed towards the surface, and the necessary sloughing and ulcerative processes are more rapidly performed.

I have also observed that the core is more extruded or pushed out than where incisions have been made. In the latter case we have often to pull at the core, and to dress the wounds with terebinthinates and other stimulating dressings before the sloughs separate. This has never been necessary where pressure was used. The core came out generally in one mass, and a healing process seemed to have been going on behind it, so that the subsequent closing up of the cavity was rapidly effected. I may add that I have frequently used the soap plaster with opium, in common furuncle, and in acne rosacea, with the best effects. It checks the suppuration of furuncle if applied in time, and always relieves the pain. In acne it seems to resolve the hard exudation, and to promote a discharge of a drop or two of healthy matter. This has been specially the case in the very chronic form of acne, which would otherwise remain for weeks itchy and sore, neither disappearing nor suppurating.

Pressure is not to supersede appropriate internal treatment, of which, if I have said little in this paper, it is not to be supposed I am neglectful.—*Dublin Quarterly Journal, Feb.* 1864, *p.* 74.

60.—ON MALIGNANT FIBRO-CELLULAR TUMOUR.

By Dr. A. B. Buchanan, Physician to the Glasgow Dispensary for Skin Diseases, Royal Infirmary, &c.

Two divisions of tumours are in common use—one into *innocent* and *malignant*, which is best adapted for the practical physician; the other into *homologous* and *heterologous*, which is best adapted for the pathologist, and which is, on the whole, the more scientific and precise. A *homologous tumour* is one, the cells of which are identical with, or only slightly differ from, those normally present in the tissue in which the tumour occurs; as in the familiar case of a fatty tumour developed in the areolar tissue. A *heterologous tumour* is one, the cells of which are quite different from those of the tissue producing them; as when epithelial cells are developed in an internal organ, and without connection with any epithelium. At the same time the division into innocent and malignant possesses obvious advantages; and fortunately both may be retained without causing much confusion. In fact, as a rule, a homologous tumour is innocent in its character, and a heterologous malignant. It is only necessary to bear in mind that there are some exceptions; because the *heterologous nature* of a tumour is *only one of the characteristics of malignancy.* In a malignant tumour or *cancer*, using the term in its widest sense—1st, The cells do not generally resemble in form those of the tissue in which they occur; 2nd, They do not generally resemble one another—as pathologists say, they are *indifferent;* 3rd, Whether these characters apply or not, they tend to propagate themselves indefinitely, and in great measure independently of the surrounding tissues, which they infiltrate without discrimination; and 4th, In a malignant growth there is a tendency to local fatty degeneration of the newly formed tissue, which on a free surface produces ulceration. Hence there are certain homologous tumours which are malignant or cancerous; and fibrous and fibro-cellular as well as glandular tumours, although not heterologous like a true carcinoma, may be divided into the innocent and the malignant. In our case the tumour is homologous in form, but malignant in its mode of development. It is even partially heterologous in form where it has attacked the muscular tissue; but still it is essentially homologous. It is an unlimited growth, according to the theory we have regarded as most probable, of cellular elements belonging to the areolar connective tissue, unaccompanied by a proportional increase of the intercellular substance; and accordingly we have named it a *malignant cellular fibroma*, or, to avoid all pedantry, a *malignant fibro-cellular tumour.—Glasgow Medical Journal, April* 1864, *p.* 57.

61.—ON THE TREATMENT OF ENLARGED BURSÆ AND GANGLIONS.

By **Frederic C. Skey**, Esq., F.R.S., President of the Royal College of Surgeons, and Surgeon to St. Bartholomew's Hospital.

The bursa in front of the knee-joint is situated partly on the lower half of the patella, and partly on the tendon of the quadriceps muscle, commonly called the ligamentum patellæ. When diseased, it exhibits the following characters. It may be small or large in size, varying from that of the half of a small apple to the size and form of a large orange. The swelling may be hard or soft, composed of thick walls of lymph, with a small central cavity, forming a mere cleft or fissure, or more commonly consisting of the bursa dilated by a collection of serum or pus. It may be active and painful, or chronic and painless. In both cases it is an evil and an inconvenience, and demands the best resources of surgery for its cure. The subjects of this disease, on applying for hospital relief, state themselves to have been under treatment for weeks or even months. The treatment in question resolves itself into two varieties—first, by the repeated application of blisters; second, by that of tincture of iodine. Both are inoperative, for at the expiration of many weeks the disease triumphs. All the above varieties of bursal disease are readily amenable to the influence of a moderately thick silk thread passed through them. The effect of the thread is the destruction of the bursa, whether composed of solid walls or fluid contents, and the formation of an abscess. The period required for this conversion varies from three to ten or twelve days. The presence or the immediate advent of matter is indicated by pain in the swelling and by the existence of a red halo around the openings made by the needle. When this sign is *fully* established, the thread may be withdrawn. The bursa is now and for ever obliterated, and we have an abscess in its place, identical with, and amenable to, the same treatment as an abscess in any other place.

The more chronic and solid varieties pass more slowly into suppuration. The more acute cases, when acompanied by redness and pain, require watching and the early removal of the thread.

Adventitious bursæ, ealled GANGLIONS, presenting on the back of the wrist, the dorsum of the foot, and elsewhere, may be successfuly treated if the contents be *entirely* evacuated; the opposite walls being maintained in absolute contact, will unite, and the ganglion be obliterated. The operation may be effectually done with a common lancet, but will fail if any, the smallest portion, of the contents remain behind. It will also fail when any portion of the cyst has forced its way underneath a tendon. A thick pad of lint applied over the cyst, and forcibly compressed by means of good plaster and a roller, will effect a cure in forty-eight hours in most cases.—*Lancet, Jan.* 2, 1864, *p.* 1.

62.—ON RANULA.

By FREDERIC C. SKEY, Esq., F.R.S., &c.

Opinions differ as to the seat and the cause of the formation. On one occasion I extracted a small portion of calcareous matter from what I presumed to have been the orifice of a duct, in a case of ranula, by the removal of which the disease was cured. The practice of laying open the cavity, and applying nitrate of silver to the interior, is adopted by many surgeons; but I have seen the disease return after the adoption of this remedy, and it is a somewhat violent remedy for so slight a disease. A thread will cure it quickly, and without pain or inconvenience. The silk should be passed through *the middle* of the cavity, otherwise the absorption of the contents is so rapid, and the adhesive process so active, that a small residue of the cyst will escape, as it were, from the thread, which occupies the situation of the former disease, now reduced to very small dimensions. The first thread should be removed, and another introduced through the remaining part of the cyst.

The large examples of ranula are rare. Commencing in the sublingual region, they extend down the side of the neck, presenting a rounded prominence of considerable size; the surface is uniform, and the skin not discoloured; the contents are obviously fluid. The portion of the cyst within the mouth is not unusually large; but the pressure of the two fingers readily detects the identity of the two swellings. In these large examples the influence of the thread is equally efficacious as in the ordinary ranula. It may be passed by means of a long needle from the mouth to nearly the lowest part of the projecting cyst and brought through the skin of the neck, and the ends tied together, the upper end being brought through the mouth. The lesser ranulæ are thus curable often in a few days; the larger require from fifteen to twenty days.—*Lancet, Jan.* 2, 1864, *p.* 2.

63.—ON CHRONIC ULCERS.

By **Frederic C. Skey**, Esq., F.R.S., &c.

The more chronic the ulcer, the larger its size, the more aged the subject, the more remarkable is the influence of opium in effecting its cure. Let a case be selected for experiment of some twenty years' duration, which has exhausted the patience of various medical attendants, as well as the remedies employed by them for its cure. Treat such a case of chronic ulcer of the largest size, having a pale flat bloodless base, a high mound of lymph around it covered by unhealthy integument, the sore pouring out a large quantity of watery ichor, saturating the linen, stockings, and other appliances—I say select such a case occurring in old age; give such a man ten or fifteen drops of tincture of opium night and morning, leave his bowels alone, and observe the base of the sore in five or six days: it will exhibit a number of minute red points, which, daily increasing in number, will rise up in the form and identity of healthy granulations, and cover the entire surface of the ulcer. Contemporaneously with the gradual elevation of the base of the ulcer is the descent of the surrounding eminence and the commencement of the process of cicatrization. If I desired to select an ulcer, on behalf of a student, with a view to illustrate the character of perfect granulations as they appear in a thoroughly healthy example, I would select an ulcer which had been treated by opium in preference to any other. If it be supposed by any man having a limited experience in the employment of opium that any evil to the constitution attaches to the use of that valuable agent, I can only reply that its salutary action on the ulcer is obtained solely through the healthy influence it exercises on the constitution. Judiciously employed, no drug in our Pharmacopœia is more innocuous.—*Lancet, Jan.* 2, 1864, *p.* 2.

DISEASES OF THE EYE AND EAR.

64.—ON THE DISEASES OF THE LACHRYMAL APPARATUS.

By **George Critchett**, Esq., Surgeon to the Royal London Ophthalmic Hospital.

[In the first place it is essential that the balance between the amount of tears, and the capabilities of the tubes in their healthy state to carry them off, be preserved. There must be no undue amount of tears, and there must be perfect patency of the canaliculi and nasal duct, and the puncta must face inwards upon the surface of the globe.]

We may first direct our attention to the abnormal conditions of the puncta and of the canaliculi. The puncta are liable to fail in the due performance of their duty from errors in position and from changes in their structure. We have already stated that in the normal condition the punctum lies in contact with the globe, and that such a position is an essential element in the proper performance of its function; but we find that it is liable to displacement, and that a very slight deviation from the normal position is sufficient to mar its efficiency—so slight as to escape observation unless carefully sought for. If the lower punctum presents upwards, so that the opening can be seen without everting the lid—a condition that is frequently produced by chronic ophthalmia—it will no longer perform its function. Or, again, the punctum may be pushed away from the eye to a greater or less distance: this I have observed as the result of hypertrophy of the inner caruncle, which mechanically pushes the inner angle of the lid away from the eye; or it may result from thickening of the lower lid and of its mucous lining. It sometimes occurs that in old cases of tinea and chronic ophthalmia with extensive eversion of the lids, the puncta present directly forwards on the cheek, and in a position where their presence would hardly be suspected, and where they become so changed and concealed that they can only be discovered by an experienced eye. Again, the puncta may become altered in their condition. Thus the delicate mucous lining may become slightly swollen and villous, and may thus mechanically obscure the opening, and fill it up so as to unfit it for the performance of its function. In some of these cases it is difficult to find the opening, more particularly if it be both displaced and choked up. Or, again, the fibro-cartilaginous opening may be contracted, so as scarcely to admit of the finest hair. Sometimes the opening of the punctum and the delicate membrane immediately surrounding it presents a white, greasy aspect, and seems to be changed into a thin cuticle; it may also be raised up into a papilla: these changes prevent the tear from flowing into the sac. The entire canal may be narrowed; or it may be closed at one point; this most frequently occurs at the point where the canal opens into the sac; though, if it result from wound, it may be found in any part. These changes seem to depend upon old ophthalmiæ, extending to these delicate parts; producing at one time a granular and villous state of the mucous membrane, at another a thickening and contraction of the submucous tissues, and stricture, as the result, just as we find in stricture of the urethra. It is possible that an acrid state of the tears may in some cases be an exciting cause, and long disuse of the canals would also seem to produce stricture. Again, small calculi are occasionally met with, filling up and enlarging the canal. I published a case of this kind that I met with in private practice in the year 1853, in a quaker lady aged about sixty. I may observe that these changes that I have described are most frequently met with in the lower punctum and canal, which seem to play a far more important part in carrying off the tears than the upper.

The symptoms vary much, according to the case. Thus if it be a case of simple displacement, or of some mechanical closure of the punctum or canal, the chief inconvenience is the watery eye; though this is often complicated with a sense of weakness and difficulty in keeping up a prolonged use of the organ. I have, however, met with cases where the inconvenience seemed to be very slight.

In those cases where stillicidium is complicated with and partly caused by ophthalmia tarsi, with its concomitants, eversion, thickening, and excoriation, the constant accumulation of tears in the palpebral fossa, the overflow of the tears, their acrid quality, and the wet condition in which all the parts are kept, tend materially to aggravate this disease, and to prevent the ordinary remedial measures from exerting a beneficial influence. It frequently happens that if the tears can be restored to their natural channel an important step is taken towards the accomplishment of a cure. I may mention here that I have met with cases in which the central part of the lower lid has been not only separated from the globe, but depressed to a level below the situation of the puncta; this creates a low-water level, into which the tears gravitate even when the puncta are fit to perform their functions. In the groups of cases that I have described, in which the puncta are either displaced or obstructed, or both, the sac and duct remain unchanged, and are fit to resume their functions even after many years of disuse. I have met with some striking cases in proof of this fact, though in some other cases long disuse seemed to have exerted an unfavourable effect upon the sac and nasal duct.

Until a comparatively recent period, these causes of epiphora were unrecognised, and their treatment consequently unattempted. In the year 1851 Mr. Bowman published a paper upon this subject, explaining both the pathology and treatment of these cases. The latter we may now proceed to consider.

In those cases in which there is simple displacement of the punctum, without any change in its structure or in that of the canal, the object is either to restore it to its natural position, or to convert the canal into an open drain so as to carry off the tears as they collect in the lacus lachrymarum at the inner canthus. In cases where the lid is pushed away by an enlargement of the caruncle, I have removed a portion of this with a good result; and in cases where the displacement was evidently due to temporary swelling of the lid, I have waited for its subsidence. But in the greater number of cases it is desirable to lay open the canal, as first suggested by Mr. Bowman. This little operation, simple as it may seem, is not altogether devoid of difficulty; it may either be performed with a fine lancet-pointed knife and a little director, or with a fine pair of scissors, or by means of a fine probe-pointed narrow bistoury. When the director and knife are used, the lid must be put on the stretch with the ring-finger of the left hand; the director must then be introduced with the right hand, and placed and held between the finger and thumb of the left, horizontally, so as to straighten and tighten the canal; the point of the knife is then introduced into the groove, and the canal is slit up. When a patient is timid, and the orbicularis muscle is put into strong action, and there is no assistant, this manœuvre is not always very easy, and it requires a careful attention to these combined movements to accomplish it. If scissors be employed, they must be fine and nearly pointed, the extreme points rounded off to prevent them from becoming entangled and lacerating the mucous lining of the canal; one blade of the scissors may be easily passed along the canal as far as the caruncle whilst it is held in the same position as when the director is used, and then with one rapid snip the canal is laid open. It is certainly more easily done in this way, and I usually adopt it in all cases where the punctum and canal are of the normal size, and are merely displaced. If the puncta are much contracted or much displaced, and concealed by a villous condition of the conjunctiva, there is often considerable difficulty in discovering the opening, and when discovered there is further difficulty in passing any instrument into the canal. Under these circumstances, I have found that a very fine director or a fine probe-pointed bistoury is more convenient than scissors. It is very important not to wound the mucous lining of the canal, except at the part where it is laid open, otherwise a traumatic stricture or closure may be produced that permanently obstructs the passage. After this has been done, it is important that the case should be seen for a few times at intervals of two or three days, and that care should be taken to prevent the reunion of the divided edges of the canal until each edge has healed. The passage of a fine probe will suffice; and if union has taken place, it readily yields before the probe. In some cases I have observed the tendency to close up very strong, and a gradual contraction to occur until the slit becomes very small, and this has occurred after repeated divisions. I have also observed the outer half or two-thirds of the canal join up, and a fistulous opening remain at the inner canthus which has sufficed for carrying off the tears. It generally happens that where displacement has been the only abnormal condition, this operation effects a perfect cure, allowing the tears to flow down the open gutter that has been thus permanently formed. Occasionally, however, I have found that the eversion is so considerable, owing to the thickened and swollen state of the lower lid and its inner lining, that even the canal, when laid open, remains everted, and the tears do not reach it, but still flow over. Under these circumstances I have been in the habit of seizing a portion of the posterior wall of the canal and snipping it out with scissors, thus effecting the treble objects, of drawing the canal more inwards towards the caruncle, of forming a reservoir into which the tears may run, and of preventing any reunion of the parts. This plan may be adopted in cases coming from a long distance and returning into the country immediately, with a view of preventing a reunion of the divided surfaces.

Of all the causes of stillicidium which we have to treat, these give the most certain and the most triumphant result. I have known cases in which the condition had existed for many years cured in a few days. When the punctum is closed or the canal strictured, the treatment becomes more complicated. Wher it is very much everted and concealed by a villous state of the mucous membrane it is difficult to find the aperture. A lens will often assist in this, and by trying with the fine director it may generally be discovered and laid open. When the canaliculus is converted into a permanently open gutter, if the tears still flow over, it is a proof that obstruction exists at some part of the little mucous tube. The situation of stricture is generally where the canal opens into the sac. This can be ascertained by passing a fine bulb probe in the direction of the canal towards the sac—that is, inwards and slightly upwards. As soon as the point of stricture is reached an elastic resistance may be felt, and as the probe is pushed on, the parts round it may be observed to move with it; whereas if the probe pass into the sac, it is brought at once against the firm, unyielding inner wall. This is a matter of considerable practical importance in determining whether or no there be a stricture. It is often a matter of difficulty with those unaccustomed to these delicate little manœuvres to ascertain if the probe has entered the sac. Attention to the symptoms I have just mentioned will greatly facilitate this point. If a stricture exist in the lower canaliculus, I always lay open the upper and try that in a similar manner, and if it be pervious throughout, it will generally suffice to carry off the tears. If a permanent obstruction to the entrance of a fine probe into the sac exists in both canals, the case assumes a more complicated and difficult character, and the result of treatment is sometimes doubtful and disappointing. If the patient experience but little inconvenience from the overflow of the tears, and if the necessity for wiping the eye occur at long intervals, I sometimes leave matters *in statu quo.* If, however, the patient be anxious for further relief, a broad, lancet-pointed needle, guarded, must be passed along the canal as far as it can be carried ; then, as soon as the point of stricture is reached, the sheath must be drawn back and the point pushed on into the sac : its direction may then be changed so as to pass it perpendicularly down the nasal duct. After the withdrawal of the canular needle, a probe must be passed through the opening that has just been made into the sac and down the nasal duct. Some difficulty is sometimes experienced in passing the probe through the opening that has been made in the outer wall of the sac, but with care and perseverance this may generally be accomplished ; if not, the opening made by the needle must be enlarged. It is important that a probe should be frequently passed into the sac and down the nasal duct, at intervals of two or three days, so as to prevent the reclosure of the opening into the lachrymal sac. It is this tendency to reclose that constitutes the difficulty in the treatment of these cases, and that sometimes exhausts the patience both of the surgeon and the sufferer. I have observed that where this form of obstruction has existed for several years, a depression may be noted in the skin over the sac. This latter seems shrunk, and its walls collapsed and fallen together and dry, having lost much of its mucous character. This change, where it exists, still further complicates and impedes the restoration of a permanent canal for the passage of the tears.

[Previously to the introduction of Mr. Bowman's plan of treating obstruction of lachrymal canals no satisfactory treatment was known. Much ingenuity had been expended and in most instances wasted on the subject. There seems to be a strong analogy between stricture of the urethra and stricture of the lachrymal and nasal ducts, and probably the only reason why dilatation of the nasal duct was never tried before, was the inaccessibility of the inlets of the lachrymal sac.]

In the Ophthalmic Hospital Reports of October, 1857, Mr. Bowman showed that the small fibro-cartilaginous aperture at the commencement of the canaliculi was in reality the only firm, contracted, undilatable part of the passage, and that when that ring is divided, a loose membranous channel is discovered that admits of some distension, and of the passage of a probe of medorate size, about the twentieth of an inch in diameter, into the sac. This valuable suggestion at once placed the treatment of lachrymal obstructions nearly on a par with stricture of the urethra. It must be admitted that the delicacy and minuteness of the parts concerned, the abruptness and angularity of the curve at the point where the canaliculi join the sac, and the osseous boundaries of the duct, throw the balance of difficulty on the side of contractions and morbid conditions of the lachrymal passages; but, at the same time, experience now abundantly shows that many of these cases are capable of permanent relief, and even of complete cure, and that in nearly every instance important benefit may be obtained. In a disease presenting such varied modifications and changes of condition and structure, it would be unfair to expect a restoration in all cases to the normal standard and function. Nevertheless, very much can now be done, even where all looks most unpromising.

In order fully to explain the method of treatment that is best suited to each stage of this disease, I propose in the first place to consider the various stages of the operation of probing or catheterizing the nasal duct in an ordinary case of obstruction of that part, and the subsequent proceedings necessary for effecting a cure. I will next consider the principal difficulties that are liable to occur, and the complications that arise; and lastly dwell briefly upon those cases in which the morbid changes are too extensive to admit of permanent relief by this simple method, and in which a more severe and radical plan of treatment seems to be indicated.

We will suppose, in the first place, that we have an ordinary and average case of nasal duct obstruction, with the sac somewhat enlarged and thickened, and secreting a glairy, mucous, or puro-mucous fluid, which, on making pressure, escapes through one or both puncta, and cannot be made to pass down the nose. The first step of the operation of catheterizing the nasal duct is the same as in those cases of displaced puncta which I have described in my first lecture on this subject, and it may be performed in the same way. The canal having been straightened and tightened by drawing the lower lid slightly downwards and outwards, and fixing it with the finger against the lower margin of the orbit, a fine pair of scissors with the points slightly rounded may be employed; one blade may be introduced into the punctum, and passed along the canaliculus as far, or nearly as far, as the caruncle, and then with one rapid snip the punctum and a portion of the canaliculus is laid open. This is the quickest and easiest method of effecting the operation. Other plans are sometimes adopted. A fine grooved director may be introduced through the punctum and along the canal, and then, the parts being drawn out firmly, a sharp narrow triangular knife may be passed along the groove. This plan is preferred by some because the canaliculus can be divided more freely and to a greater length. It is even recommended that the director should be passed into the sac, and that a long narrow knife should be carried on into the sac, so as to facilitate the subsequent passing of the probe down the nasal duct. It is, however, only under peculiar and exceptional circumstances that such a proceeding is desirable. It endangers a wounding of the floor of the mucous canal, and the formation of a traumatic stricture. A mucous membrane may be regarded as a very sacred thing; when once injured or destroyed, its place cannot be supplied. No efficient substitute can be found; and although some false passage may be formed, it never can retain the properties of the original mucous tube. This is especially true as regards the delicate aqueducts that convey the tears. Great care, gentleness, and patience are required in the performance of the little operation I am now describing. Another method of opening the canaliculus is to introduce a very fine probe-pointed bistoury, and by a slight sawing upward movement lay it open. In practice I generally employ scissors.

The next stage of the proceeding is the passing the probe. Previous to doing this, it is well to observe whether the contents of the sac regurgitate through one or both puncta; and if only one, whether it be the upper or lower. If the upper, as is not unfrequently the case, it should be laid open in a similar manner to the lower. It is, perhaps, rather more difficult to accomplish than the lower, especially when the patient is nervous and irritable; but a little practice gives the necessary dexterity. As we might *à priori* expect, it is generally found that the canaliculus that permits regurgitation most readily, admits a probe most easily. It is better to be provided with a set of probes graduated of different sizes. Mr. Bowman's set of probes represent a graduated scale of six sizes. Mr. Teale, jun., of Leeds, in a paper which he published on this subject, recommended the use of bulbed probes made upon a similar graduated scale. I usually employ the bulbed probes; I think they pass more readily, and are less liable to lacerate the mucous lining, or to get into a wrong direction and make a false passage.* I usually commence with a probe of medium size. Whichever canaliculus is selected, it should be drawn outwards firmly so as to form a straight tunnel along which the probe may pass. The probe must be carefully introduced into the loose membranous canaliculus, and then gently glided on into the sac; care being taken, as it passes from the open into the closed part of the canal, that it does not pass into the loose cellular tissue, and thus wander away from the sac and form a false passage. If it passes smoothly on till it strikes against a firm resisting wall, it has entered the sac. The direction of the probe up to this time must be inwards and slightly upwards, if it be the lower passage; if the upper be selected, then downwards and inwards. As soon as the probe has entered the sac its direction must be entirely changed. The point of the probe resting in the sac, the handle must be moved in a circle until it is brought on a line with the nasal duct; it must then be gently pressed downwards so as to enter the nasal duct. This requires care and management. If it be felt to strike against some hard resisting medium, it has failed to enter the duct, and is in contact with the bony wall. It must then be slightly withdrawn, its direction must be changed, and it may be partially rotated so as to bring the bearing upon various points until it yields, or gives only an elastic resistance which a little pressure will overcome. The strictured point once passed, the probe will generally pass very readily down to the floor of the nose. This once accomplished, a very important step towards cure has been gained. It will generally be found that the contents of the sac can be pressed down the duct, instead of regurgitating; and the patient should be directed to empty the sac frequently, and, if possible, downwards into the nose. This facility in emptying the sac is a great advantage in the restoration of parts to a healthy state. The probe that has been passed may be left in for a short time, and in three or four days the operation may be repeated, and an attempt may be made to pass a larger size. Gradually the sac will resume its normal dimensions, the secretion from it will cease, the tears will flow in the right direction, and the case will be relieved. A tendency from time to time to relapse is not unfrequent; but an occasional passage of a probe will set everything right again.

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* These probes, and the various instruments required in the treatment of these cases, can be procured at Weiss and Son's, Strand.

We may now consider the principal difficulties that are liable to occur, and the complications that arise in the treatment of these cases. The first condition of this kind to which I would direct attention is a contracted, unyielding state of the canaliculi. Unless this can in some way be overcome, it seriously impedes the subsequent treatment of the case. We may endeavour to meet the difficulty, either by trying to stretch and dilate the passage, or by laying its entire length open upon a director as far as the sac. The dilatation may be effected by means of a delicately-formed instrument, made by Weiss at the suggestion of Mr. Bowman, upon the principle of the female urethra dilator. I sometimes employ for the same purpose a material respecting which I shall have more to say subsequently, and which has the double advantage, that it can be fined down to almost any degree of tenuity, and when introduced into the canal, and subjected to moisture, will gradually increase to three or four times its previous diameter. In this way, by care and patience, a sufficient space may be obtained to allow a probe of average size to be passed. I rather prefer this method to any more forcible and rapid proceeding, as I consider it better to stretch than to tear and lacerate the membrane. If the object cannot be gained in this way, a long, narrow, finely probe-pointed bistoury must be passed along the canaliculus into the sac, and then, by a gentle sawing movement, the entire length of the tube must be opened and enlarged. I avoid this proceeding as much as possible, for although it facilitates the subsequent passage of a full-sized probe, it is often followed during the healing process by contraction more troublesome than the previous condition. We also meet with cases in which it is impossible to pass a probe or even a fine director into the distended sac. This may arise from a change in the direction of the canaliculus, causing it to curve backwards somewhat behind the sac, and making a sort of valvular closure, as evidenced by the slight regurgitation of fluid on pressure. Or the communication with the sac may be quite closed, and in that case the contents may either be made to pass by means of firm pressure down the duct, or in some instances it may be completely shut in. Under these circumstances a canular needle must be passed along in the direction of the sac, and made to penetrate its wall.

Another source of difficulty in the treatment of these cases is the tightness and firmness of the strictured part or parts, and the strong tendency they show to reclose after the passage of a probe. Various contrivances have been devised in order permanently to overcome the stricture. A modification of the old-fashioned style has been passed in a similar manner to the probe, and left in for a period varying from a few days to a few weeks in the hope of establishing a free and permanent passage. Experience, however, proves that there are serious objections to this plan; it often creates considerable pain, irritation, and even inflammation, during its residence in the canal, and, by the pressure it exerts upon the mucous lining, causes ulceration and destruction of this membrane. The result of this is not uncommonly a firmer and more invincible contraction than before, the treatment of which is much more difficult because the greater part of the true mucous lining is gone, and can never be restored.

In such cases as these, I have recently been in the habit of employing small bougies, formed of a species of sea-weed, the dried stem of sea-tangle—*Laminaria digitata.* This substance was first brought before the notice of the profession by Dr.

Sloan, of Ayr, in the Glasgow Medical Journal for October, 1862.* It possesses several important advantages: it can be worked up of any size and length; it is quite smooth, firm, and elastic in its dry state, so that it can be passed very nearly as readily as a silver probe. When exposed to moisture, it expands to at least double, and even treble its former size. A power such as this, cautiously and skilfully employed, may be rendered of great value. I have succeeded in conquering some very obstinate and inveterate cases of long standing. I generally commence with a rather thin, fine bougie, that will readily pass, and I leave it in about ten minutes. At the end of that time it usually offers some resistance as it is drawn out; it is swollen, and at one or more points it is contracted, showing the seat and extent of the stricture. This is usually found to be a little below the sac, at the commencement of the duct, or only a short way beyond this point. Another bougie may now be used, partly covered with copal varnish in such a way as to limit the dilatation as much as possible to the strictured part. By gradually increasing the size of the dilatable part, and by leaving it in a somewhat longer period,—twenty minutes or half an hour,—a powerful effect may be produced upon the constricted portion of the canal. In fact, the principal risk in the employment of this material consists in the excess of power you possess. If a bougie be introduced of such a size as to pass in rather tightly, and if it be left in the canal for half an hour it will be found to have expanded to such a degree beyond the seat of stricture that its removal is attended with considerable difficulty, and with some violence to the strictured part, as the swollen extremity forces its way through it. This, however, is an excess in the right direction. Every good mechanic knows that if he has a superabundance of force, he has only to subdue it to his will and regulate the direction of it in order to obtain from it a slavish and useful action. It is, in fact, the only way in which the principle of gradual dilatation can be applied with advantage to strictures of the nasal duct. I have found it very useful, and I believe it is capable of further development. It will not, of course, supersede the use of probes, but it will co-operate with them.

Another point in the treatment of these cases is the extent to which the sac becomes altered both in size and condition. These changes I described in the last lecture. Where, for instance, the sac has become very much dilated, thinned, and distended with glairy fluid, it is often found that after the stricture has been relieved, and when the contents of the sac can be readily pressed out both through the canaliculi and down the nasal duct, the lax and dilated walls of the sac quickly refill, and do not contract to anything like their normal dimensions. The patient should be directed to keep the sac constantly emptied by pressure with the finger, and as much as possible to keep up the pressure, so as never to allow the cavity to become distended. It is, however, found in practice, that, in spite of every precaution, this will frequently occur, particularly during the night. It is therefore in such cases that some mechanical means of keeping up gentle pressure on the anterior wall of the sac are often useful. I have sometimes employed a truss for this purpose, with a frame-work fixed on the forehead, and an arm coming from it, and so adjusted as to keep up gentle and continuous pressure. In another class of cases the sac is very much thickened, as well as enlarged, the lining membrane villous, and secreting puro-mucous fluid in considerable quantities. A restoration of the nasal duct will not remove the morbid condition of these parts. A similar treatment to that of the last group will be of some value. The injection of a stimulating and astringent lotion into the sac by means of a syringe is useful; but in severe cases something more than this is required. On one or two occasions Mr. Bowman has dissected out the anterior part of the thickened sac with good effect, the only objection being that it is rather a tedious and difficult operation. Instead of this, I have sometimes freely opened the sac, and applied the potassa cum calce to the interior so as to destroy a considerable portion of it, taking care not to injure the skin. The cavity thus formed gradually granulates and heals, leaving a scar that is scarcely perceptible, and a sufficient space for the tears to find their way into the nose; or it may be necessary to renew the treatment by probes. The case will be much simplified by the destruction of a large portion of the diseased and thickened sac.

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* My attention was first directed to this substance by my friend and colleague, Mr. Couper, of the London Hospital, who has made some interesting experiments upon it. The bougies may be obtained at Kröne's, surgical instrument-maker, Whitechapel.

Another complication which I have described in the last lecture is a second cavity, communicating with the original sac, and frequently with the external parts, by means of a sinus. After the stricture has been relieved by probing, it is observed in such cases that a fluctuating tumour still remains, which cannot be emptied by pressure. We may then suspect the presence of a second cavity. This must be freely laid open, caustic applied, filled with lint, and healed from the bottom.

Cases of true fistula lachrymalis, in which there is an aperture in the skin, through which the tears constantly flow, is rather uncommon, and is not very easy to manage. We must endeavour, in the first place, to restore the permeability of the nasal duct in the usual way, and then to close the fistulous aperture by paring the edges of the skin, and bringing them together with fine sutures, so as, if possible, to obtain union by the first intention.

In cases of acute abscess of the sac, I have sometimes succeeded at an early stage, and just as matter is forming, in checking its further progress by freely opening the canaliculi, and passing a probe into the nose, so as to make free inlets and outlets, to the sac. At a later stage this becomes impossible, and a free opening must be made through the skin into the sac. No attempt can be made to restore the duct until all inflammation has passed off; but the occurrence of an acute attack renders the success by this method less probable.—*Lancet, Dec.* 19, 1863, *Feb.* 6, 1864, *pp.* 697, 147.

65.—ON A NEW MODE OF APPLYING SOME EXTERNAL AGENTS TO THE EYE.

By J. F. Streatfeild, Esq., Assistant-Surgeon to the Royal London Ophthalmic Hospital.

The use of paper as a medium for the application of sulphate of atropine and various other soluble salts to the eye has been before the public since my communication on the subject in the Ophthalmic Hospital Reports, in the number for January, 1862. This medium has been very much approved, and I may say generally adopted, owing to its convenience, and more or less also owing to its peculiar efficacy in some cases.* It has occurred to me that an improvement might be made if, in the new medium, we could secure, along with the convenience and efficacy pertaining to the paper, some vehicle which would not need to be removed from the eye after it had been applied, and the desired result had been gained. I have not now time or space even briefly to relate the advantages arising from, or the way of making the application by, the paper vehicle. I can only refer to my communication above mentioned, and to another in the succeeding number of the same journal. But I may allude to the alarm felt by some patients at any investigation of their eyes, even if only to remove the scrap of paper that has been introduced, and to the trouble sometimes found in removing it when it has got up beneath the upper lid. As the proposed substitute for paper need not be removed, the surgeon will often be saved a delay of twenty minutes, and will not be obliged to wait for the desired effect in order to remove the scrap of paper. Therefore, also, the patient can be trusted to employ the new medium himself, which is not the case with the old one. In order to obtain what I wanted, I first tried what is called "wafer-paper;" but this is brittle when dry, and loses all consistence when wet, so that it becomes unmanageable as soon as it is moistened; and moreover, the pulpy substance in the eye, if not washed out, might irritate. I am now employing gelatine, rolled out in sheets of the thickness of thin writing-paper, and imbued with a definite proportion of sulphate of atropine or any other salt required. This retains the advantages of, and only requires the same mode of application as, atropine paper, but *it need not be removed from the eye.* It is soon dissolved in the tears, and acts in every way well. The dose employed thus, it appears, is more quickly applied to the eye than when a similar dose has to be dissolved out of the meshes of tissue-paper; and to this it is that I attribute the pain felt by some of my patients when I have used a large square of "gelatine atropine paper." They have not complained of pain when I have used a smaller bit (or dose), less than a whole square. I am thinking of having a check of small squares printed in green on the gelatine, to define the doses, and to make the transparent gelatine more readily visible, *when about to be used*, for it need not be looked for afterwards.*

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* The method was *invented and patented* in Paris a year after it was published in London.

[We believe that Mr. Streatfeild has the honour of being the first to suggest this kind of practice, which we think valuable. EDS.]—*British and Foreign Medico-Chirurgical Review, Jan.* 1864, *p.* 281.

66.—SUBSTITUTION OF ATROPIZED AND CALABARIZED GELATINE FOR PAPER, FOR THE PURPOSE OF DILATING OR CONTRACTING THE IRIS.

By ERNEST HART, Esq., Ophthalmic Surgeon to St. Mary's Hospital.

In a recent record of the practice of Mr. Hart, we mentioned that he had caused to be prepared atropine papers considerably weaker than those originally prepared for Mr. Streatfeild, by Mr. Squire. The latter were of a strength such that each square contains an amount of atropine equal to that contained in one drop of a solution of two grains to an ounce (240th of a grain). For ordinary, and especially for ophthalmoscopic purposes, this is an inconvenient strength; it produces not only dilatation of the pupil, but paralysis of the accommodation of the eye, with the consequent inconvenience of inability to adjust vision for near objects, and dazzling from the disparity of the visual powers of the two eyes. The papers prepared by Messrs. Savory and Moore for Mr. Hart run as low as 100,000th of a grain in each square. With this strength dilatation is secured from ten to fifteen minutes; and, when more time can be given, even feebler doses may be used. The dilatation, even with the 100,000th of a grain, will often last for as long as twenty-four hours if unchecked, although, as accommodation is very little affected, there is but very trifling inconvenience indeed attaching to this condition. The Calabar bean papers of Squire and Bell do not suffice to overcome this dilatation satisfactorily if only one square is used; two squares commonly suffice. A stronger paper of Savory and Moore effects the recontraction with one square, if this be desired. These facts have been repeatedly tested on the patients in the out department, and especially in preparing them for ophthalmoscopic demonstrations.

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* It is made by Mr. Squire, chemist to her Majesty, 277, Oxford-street, London, and will be shortly ready for the trial I hope it will obtain of my professional friends.

Thus as to the strength of the substances which may be employed for the dilatation or contraction of the pupil in ophthalmic practice. But Mr. Hart has found, as have other ophthalmic surgeons, that the substance employed to hold the atropine is objectionable. The use of these papers is not without inconveniences. Many eyes resent vigorously the sojourn of a foreign body between the lids and the globe, however delicately thin and small it be; and often the paper will produce so much irritation that the patient cannot endure its use. Hence, probably, for hospital practice, and in many traumatic and inflammatory cases the use of the very dilute solution of sulphate of atropine (when indicated), in distilled water, will be always preferable to the "dry medication." Obviously, however, one great objection to the use of atropine and Calabar bean in this very portable and exactly-measured dry form would be overcome, if for paper could be substituted a substance which should be equally portable and clean, but should be soluble in the secretions of the eye, and would dissolve after being placed within the lids. Mr. Hart has directed his attention to this point. Various substances occur to the mind which may be employed for the purpose, but it has not been easy to select one fulfilling all the indications. A preparation similar to that known as wafer-paper came near to supplying the desideratum, but it was difficult to get it sufficiently thin and pliable. Scales of sugar of milk can be prepared, which are convenient; but the heat required to prepare them is greater than that by which atropine is decomposed. In the end, however, Mr. Hart has fixed upon a substance which seems to fulfil the various indications very satisfactorily. This is gelatine. The ordinary sheets of gelatine are by far too thick and clumsy to be used for this purpose; but Messrs. Savory and Moore have supplied him with prepared sheets of a gelatine, very thin, pure, flexible and tough, which quickly dissolves when a morsel is placed within the eyelids. Impregnated with atropine or Calabar extract in given quantities, this fulfils all the purposes of, and, in so far as it is soluble, has a manifest advantage over the paper.—*Lancet, Jan.* 16, 1864, *p.* 65.

67.—TREATMENT OF CILIARY BLEPHARITIS BY PAINTING THE EDGE OF THE LIDS WITH TINCTURE OF IODINE.

Dr. **Fano**, Professor in the Faculty of Paris, has derived great advantages from this treatment with three children whose cases he states. The following remarks show in what way the author applies the tincture of iodine on the diseased lids, and the therapeutical effects of this application. Ciliary blepharitis is an affection with well-marked symptoms. Studied with care in our time by a great number of ophthalmic surgeons, all acknowledge now that in the beginning the disease consists of a phlegmasia of the skin in the ciliary region, and that it is only later that this inflammation, gaining the cavity of the hair follicles, causes ulcerations more or less deep, which finish by making the hairs drops out, and cause the deformity known by the name of madarosis, ciliary alopecia.

The application of the tincture of iodine on the affected parts modifies their vitality, changes the nature of the morbid secretion, and arrests the scabbing process. This application requires great care and precaution, the absence of which might cause a failure in this mode of treatment.

The scabs which cover the margin of the lid must first be taken off. If this precaution is not taken, the tincture does not act on the diseased parts covered by scabs. If these scabs are too hard to be taken off, they must first be softened with warm water or by applying a linseed-meal poultice to them. Then put a hair pencil in a solution of equal parts of tincture of iodine and distilled water, and pass the extremity of the pencil on the edge of the lid, taking care that the liquid penetrates into the interval of the lashes. The hairs of the pencil must, therefore, pass and repass several times over the same parts, without which the liquid penetrates the lashes, and not the skin. It is a regular painting that must be done.

During this application care must be taken not to touch the conjunctiva with the tincture of iodine. It is easy to do this by the following method: If it is the upper lid that is under treatment, tell the patient to look down, draw the upper lid in the same direction, so as to bring it slightly opposite the inferior one, which then covers the ball of the eye. For the lower lid, it is sufficient to draw it downwards by direct traction on the skin. If some drops of tincture get behind the lids on the conjunctiva, it should immediately be washed with cold water.—*Dublin Medical Press, March* 2, 1864, *p.* 222.

68.—ON THE ACTION, PREPARATIONS, AND USE OF THE CALABAR BEAN.

By THOMAS NUNNELEY, Esq., Surgeon to the Leeds General Eye and Ear Infirmary.

As to the energetic local action of Calabar bean upon the pupil there is not the smallest room for doubt. There is no other substance known which possesses anything like the power it does in painlessly, quickly, and without irritation, contracting the pupil. In all cases where the pupil is preternaturally dilated and it is thought to be desirable to contract it, preparations of the Calabar bean may be employed without hesitation.

It is in contracting the pupil so as to withdraw a prolapsed iris which has escaped through a wound in the cornea that I consider the great value of the bean consists. I pointed out this use in a paper inserted in the Lancet for July 18th, 1863, where I related two cases in which I had so employed it. Since then I have used it in similar cases with considerable advantage. I believe it here promises to be of the greatest value, and that it will enable us to save as useful organs many eyes which otherwise would be altogether lost or comparatively useless, and in a great number of cases to convert what hitherto has been a prolonged, painful, and very troublesome affection into a much more manageable and shorter one. So also it occurs to me that in all probability it may be valuable in those most annoying cases where after extraction of the lens the iris prolapses, and will not remain out of the lips of the corneal wound. There are few ophthalmic surgeons who have not occasionally had the mortification, after a most successful extraction, of finding the iris has prolapsed, and uncontrollable destructive inflammation has followed. Very recently, as I was operating upon an excitable woman, as soon as the knife had well penetrated the cornea, she jumped up and could not be restrained. The knife had to be withdrawn, on which the iris prolapsed. I thought this a good opportunity of trying the effect of the bean. A portion of Squire's paper was immediately introduced under the lid, and the woman placed in bed. On the day following the wound had healed, the pupil being as round and the iris as natural as ever it was.

In granular and irritable lids; in strumous ophthalmia where there is much photophobia; in some of the active inflammatory affections of the conjunctiva, where there is much pain and excitement, and where the contractile effect upon the iris will not be objectionable; possibly also in pure and uncomplicated retinitis, where contraction of the pupil by the exclusion of light would be useful,—a lotion of the extract of the bean may be valuable. So soon as I can obtain a sufficient supply of the bean to test it on a large scale, I shall put it to the proof in these and similar affections.

With regard to the best preparation and the best method of applying it, that one must be selected which will keep unchanged, and which admits of application to the conjunctiva without causing irritation; for in all cases the application should, I think, be made to this membrane. As the watery infusions and extracts will not keep, and are not so active as the spirituous preparations, they are not likely to be so much employed as them. Perhaps, if the bean becomes abundant an infusion may be used as a collyrium.

It will be seen that ether, chloroform, and acids are not good menstrua, as they possess the double disadvantage of not extracting much of the active principle of the bean, and also of irritating the eye when put into it. Their employment may be dismissed as injudicious. Boiling spirit extracts in the greatest degree the active principle; but then it is necessary to either drive off the whole of the spirit, or to so dilute this that when applied it shall not stimulate the conjunctiva. To preserve the extract and dilute it for use, glycerine has been employed. Messrs. Bell and others have enclosed the glycerine solution in capillary tubes, hermetically sealed, in which it will probably keep unchanged for any length of time. So prepared, the application is easy, by breaking off both ends of the tube and blowing out the contents into the eye; but, as the quantity put in cannot always be accurately controlled, this is an objection to the method.

I have kept the spirit extract dissolved in glycerine for upwards of three months; it is apparently unchanged, and if it so continues, as it probably will do, this forms one of the most convenient preparations. The quantity and strength may be regulated with the greatest nicety, and the glycerine is a bland medium, and very miscible with other fluids if it be wished to use it in combination or greatly diluted.

Mr. Squire and Messrs. Bell and Co. have prepared paper saturated with a tincture of the bean, and then dried like the atropine paper suggested by Mr. Streatfeild. This is a very convenient form of preparation, and should it be found to keep unimpaired, of which I have some doubt (though experience alone can determine this point), it will, from its portability, be most commonly used. When simple temporary closure of the pupil is desired, a morsel of the paper should be put within the lower lid. The principal objection which I have found to the paper is, that in some eyes it acts as a foreign substance, particularly if it be not carefully and smoothly put in. It then causes a copious flow of tears, and, being carried about by the motion of the lids, is so soon washed out that the effect is lost. I have seen the paper remain in for forty-eight hours without exciting the least irritation; and I have known it washed out in a few minutes. In one case I have known it remain in for fourteen days, and in another for twenty-one days. The thinner the paper the better it is; hence I prefer that prepared by Mr. Squire, which is not more that half as thick as that made by Messrs. Bell and Co.

Mr. Squire has supplied me with a tincture of the bean. This is an elegant and persistent preparation, but which requires considerable dilution in its use, lest the spirit prove injurious. One drop to ten drops of water will, in many cases, act efficiently. If a permanent action be required, whatever the preparation used may be, unless, perhaps, in a less degree, the paper, repetition of the application, at intervals of eight or twelve hours, is necessary in all cases. I have found the effect in twelve hours very considerably lessened, and in twenty-four, nearly, if not altogether gone. The frequent repetition does not appear to be in any way prejudicial, nor the effect to be weakened. I have kept the irides of men and animals in a constant state of contraction for days and weeks, and in one boy for upwards of a month. On the cessation of the application the pupil at once resumed its normal condition.

Whether any other substance possesses similar control over the iris remains to be proved; probably others will be found to act similarly.

As opium taken internally has the effect of contracting the pupil, I have tried its local action by dropping into the eye a solution of the acetate of morphia. A good deal of lachrymation, but not much contraction of the pupil, was induced, presenting thus a striking contrast to the Calabar bean; for while this, applied locally, contracts the pupil intensely, when taken internally, even to a fatal dose, it has little influence over it. Opium, on the other hand, taken internally, contracts the pupil strongly; applied locally, it appears to have little effect on it.

It has been stated that conia and Calabar bean have analogous actions. I have lately tried the effect of conia, locally, and internally, without confirmation of the statement. My previous experience of the action of conia when administered by the mouth or lungs had not led me to consider the symptoms induced by Calabar bean like those caused by a poisonous dose of conia. In the latter the action is much more speedy; there is coma, preceded by intense giddiness; consciousness and sensation are lost; and convulsions not unfrequently are caused,—none of which occur in Calabar bean poisoning. When conia is put into the eye great lachrymation, irritation, and photophobia are induced but no contraction of the pupil.

Mr. Reynolds suggested theine, and kindly supplied me with some for experiment. On trial, I find the result altogether negative.

Mr. Squire having sent me a supply of the extract of the flowering plant of aconite, I have tried it. It dilates the pupil less actively than belladonna, but with more irritation.

During the time this essay has been in course of publication I have had the opportunity of using the extract of the bean in the following case :—

In a child three weeks old, who has been suffering from acute purulent ophthalmia from within a few days after birth, I found a considerable portion of both corneæ in a state of slough, through which a very large portion of the irides prolapsed. A solution of the spirit extract of the Calabar bean was put into both eyes, night and morning, for two days, when the prolapsed portions had greatly receded. The extract was now suspended for two days, at the end of which time the prolapsed portions had so greatly increased in size that the employment of it was resumed at once. The escaped parts again began to recede, and the corneal wounds to lessen, until at the expiration of seven days, the ulcers had so nearly healed that the use of the bean was omitted. During this period the extract was dropped into the eyes twice daily; and as there was still some purulent discharge, a weak solution of nitrate of silver was also employed twice a day,—the two preparations being used alternately, at as nearly equal intervals of time as possible. The case is still under observation, and though it would be improper from a single case to make any very positive assertions as to cause and effect, I may with confidence state that I never saw a case in which the prolapse so completely and quickly withdrew and the corneal ulcers closed. Though the *propter hoc* may possibly not be proved, of the fact of the receding of the irides on the first application of the extract, of the renewed prolapse on the intermission of the application, and of the again speedy withdrawal of the escaped portions and healing of the ulcers on the renewed application, neither the intelligent surgeon whose patient the child was, and who himself kindly undertook to put the extract into the eyes to secure its being efficiently done, has, any more than myself, the slightest doubt. Should experience confirm my anticipations, I need hardly say that a most valuable remedy is discovered for assisting us in mitigating a dreadful calamity; for, by the speedy withdrawal of the escaped iris from the wound, there is a much greater probability of a useful pupil being left; and also, as the principal cause of irritation (the prolapsed iris) is removed, the inflammation quickly subsides and the corneal opening closes.

By way of postscript I would wish to mention that at the commencement of this essay I believe I was led into a mistake in stating that Dr. Fraser undertook his investigations at the suggestion of Dr. Christison. I have now reason to believe they were commenced *motu propriâ*, and that his thesis was printed some time before he held his present appointment as assistant to the learned Professor. This correction may perhaps not to some appear to be of much moment. I believe, however, it is only proper that it should be made. So also I find, on referring to Dr. Robertson's paper, in which he at an early stage called attention to the contractile power over the iris which the bean possesses, he suggested on theoretical grounds, that the use of it might be found beneficial in ulcers of the cornea, where there is a tendency of the iris to escape. To this Dr. Robertson has called my attention; but I find that he, like myself, had unknowingly been anticipated in the idea by Dr. Fraser in his thesis. Neither of these gentlemen, however, appears to have done more than suggest the possibility of the bean being found useful when so employed, and I feel assured that both of them will willingly concede that I may claim priority in the practical employment of the bean, as pointed out in my former paper, in wounds of the cornea, as well as in the various other affections I have alluded to in this essay.—*Lancet*, *Nov.* 28, 1863, *p.* 616.

69.—PROLAPSE OF THE IRIS THROUGH A WOUND OF THE CORNEA; DILATATION OF THE PUPIL WITH $\frac{1}{100000}$ OF A GRAIN OF ATROPINE, AND RECONTRACTION WITH THE EXTRACT OF THE CALABAR BEAN.

By Ernest Hart, Esq., Ophthalmic Surgeon to St. Mary's Hospital.

The earliest experience of the power which the Calabar bean possesses of contracting the pupil, caused its application to prolapse of the iris in corneal wound and to the recontraction of a dilated pupil to be at once indicated. In many cases of prolapsed iris it has proved of singular benefit, and in one very marked case last week at St. Mary's Hospital, it saved from excision a large portion of iris which must have been removed. A little boy had received a clean cut, which had opened the cornea freely near the sclerotical junction, and without inflicting other injury. The lad was placed under chloroform in the theatre, and the eye carefully examined, which, as he was so excessively restless, could not otherwise be satisfactorily accomplished. The wound proved to be so shaped as to leave loose a small triangular flap of cornea. Fully a fourth of the iris prolapsed. A small square of Calabar bean paper had an hour previously been introduced under the eyelids, and all the pupil was firmly contracted except the prolapsed part; the traction was not sufficient to draw this in. It was returned by a probe, and a further drop of the glycerine extract of J. Bell and Co. (one drop equal to four grains of the extract) was introduced. The lid was closed, and a pad strapped over it. All went well next morning when the eye was examined, but as the effect of the bean wore off the prolapse again occurred, as the small corneal flap had not united. Fresh instillation of the Calabar bean was made, and the eye rebandaged. By continuing this treatment, reduction has been fully established.

Mr. Hart has also been carrying out a somewhat lengthened series of observations on patients submitted to the ophthalmoscope, of which the object has been to ascertain the smallest quantity of atropine which will effect the dilatation of the pupil. The reason for wishing to ascertain this, he states, is twofold. The atropine paper prepared by Mr. Squire for Mr. Streatfeild he had found by far too strong for ordinary ophthalmoscopic purposes, and far stronger, indeed, than is generally required. Thus it not only rapidly dilates the pupil, but paralyzes completely the accommodation of the eye; and these very inconvenient conditions, with the consequent disturbance of vision, often last as long as five, six, or more days after the use of this paper. Moreover, the Calabar bean paper hitherto prepared has not proved sufficiently strong to counterbalance the degree of dilatation. The square of an eighth of an inch of this paper contains $\frac{1}{240}$ of a grain of sulphate of atropia. Mr. Hart has been using paper so prepared that one square contains as little as $\frac{1}{20000}$ or $\frac{1}{100000}$ of a grain, and he finds that dilatation is very effectually procured with the latter. The dilatation is more rapid when the eye is kept in the dark immediately after the introduction of the paper, but it takes place quickly enough when the eye is exposed to the light, and lasts nearly twenty-four hours. The accommodation is very slightly affected when this paper is used, and recontraction may then be effectually obtained with the Calabar bean paper. Mr. Hart is continuing the experiments, which will enable him to fix the lowest available strength of the atropine paper or solution, and the relative strength of the Calabar bean paper or solution. Meantime it is evident that the problem of concurrent dilatation and recontraction is solved, and that surgeons wishing to dilate the pupil will find an advantage in employing the extremely dilute strength above mentioned. The papers used in the above experiments were supplied by Messrs. Savory and Moore.

One of the difficulties of making Calabar bean paper of considerable strength arises from the fact that at present the alcoholic extract is the most condensed preparation yet obtained, containing its active principle; and this is of course far more bulky than the alkaloid atropine. Dr. Christison has for some time, Mr. Hart observed, been endeavouring to extract from the Calabar bean an alkaloid or other active principle. This, however, has not yet been isolated, and it seems difficult to ascertain even what is the nature of this active principle. The bean does not yield it to acid solutions; and hence Dr. Squire has suggested that it is probably not an alkaloid, or some salt of it would be soluble in water, which appears not to be the case, as aqueous solutions of the Calabar bean are inert. Hence it seems probable that it is some body of the nature of santonine or piperine, which belong to the class of neutral or slightly acid bodies; or it may even be a resin. But it is hardly probable that the bean will yield an alkaloid; it yields a beautiful gum. —*Lancet, Dec,* 12, 1863, *p.* 677.

70.—ON IRIDECTOMY.

By George Chritchett, Esq., Surgeon to the Moorfields Ophthalmic Hospital.

The disease has been so admirably described by Von Gräfe, by Mr. Bowman, and others, that I do not think it desirable to enter at any length upon this part of the subject. I may, however, briefly state that under the name of glaucoma we now include a great number of conditions; and that, whilst we recognise a common law binding these together, we find an ever increasing diversity of manifestation. Thus glaucoma may exhibit all the symptoms of the most acute inflammation of the globe, attended with agonising pain, sickness, and prostration, and destroying sight in twenty-four hours; or there may be a slow form of inflammation, extending over many months, sometimes proceeding steadily on until sight is extinct, at others presenting temporary remissions and ameliorations, which are sooner or later followed by relapses; or, again, we frequently meet with cases in which the sight is slowly pressed out of the eye, until vision is totally gone, without the least evidence of inflammatory action, and in such cases the interval between the commencement of dimness and the total loss of sight may sometimes be reckoned by years. Between these extremes we have every shade of difference; and the common condition or law that binds them all together is *increased tension or hardness of the eyeball.* Even the opponents of iridectomy seem to admit that glaucoma is incurable by ordinary treatment. I can well remember the pain I often experienced, and the helplessness I felt, when these cases came under my care, before iridectomy had been discovered. A case would often present itself in which one eye was hopelessly gone, utterly disorganised. The same disease would be commencing in the other eye. Symptoms would perhaps remit; would recur with increased violence; treatment, however varied or active, would utterly fail; hopeless, permanent blindness would bring the sad drama to a close; the black curtain would fall, never to rise again. In an experience of twenty years, I do not recall a single exception to this sad result. Every ophthalmic surgeon, who possesses a wide field of observation, knows that the painful picture is drawn from life. Well do I remember the half hopeful, half doubting joy with which I heard that a remedy had been discovered for glaucoma. The reports of Von Gräfe's brilliant and immortal discovery came to us at first like an uncertain sound; it seemed too good to be true. Mr. Bowman was the first to perform the operation of iridectomy in this country, at the Ophthalmic Hospital. Shortly after this, in June 1857, I performed my first iridectomy upon a lady I saw in consultation with Mr. Goude, of Cheapside. I published this, together with three other cases of acute glaucoma treated in a similar way, in the Ophthalmic Hospital Reports, January 1858. Another lady, upon whom I operated in September 1857, and whose case is also published in the same paper, called upon me a few days ago, after an interval of six years, in the enjoyment of good sight—proving not only the efficacy, but the permanent effects of the operation. Since that time, I have been in the constant habit of performing this operation. Scarcely a week has passed without giving me an opportunity of testing its merits. After having operated upon above two hundred cases, I can confidently state that the results have been most satisfactory; and that glaucoma may be pronounced a curable disease, when the operation is performed at a sufficiently early stage. As there is universality in the law that binds all cases of glaucoma together—viz., *increased hardness or tension of the eyeball*—so is there universality as regards the effect of iridectomy on the glaucomatous eye—viz., *it restores to it its natural elasticity.*

The actual effect of the operation upon vision varies greatly, according to the special circumstances of each case. Of these effects, time and space only permit me to give a very brief summary, in confirmation of statements made by other authorities and as the result of my own experience. All observers admit, that it is in acute glaucoma that the results of iridectomy are most brilliant. As the eye is speedily and violently destroyed by this form of the disease, so does iridectomy, if performed within a few days of the attack, restore the sight to something very nearly approaching the normal standard. But, although the effects of the operation upon other phases of the disease, and in more chronic cases, are less striking, they are not less certain or satisfactory, if compared with the alternative that awaits the patient when iridectomy is not performed.

In all cases of chronic glaucoma, past experience warrants the expectation that the amount of sight existing at the time of the operation will be preserved; moreover, the restoration of elasticity to the eyeball, gives to the partially paralysed retina the best chance of recovering some of its functions and sensitiveness. I have met with several interesting cases of this kind, in which the field of vision has gradually extended, and the acuteness of sight has gradually increased for many months, and even for years. In those cases in which there has been slow limitation of the field of vision without inflammation, the result of simple pressure, iridectomy arrests the disease and preserves all the sight which remained at the time of the operation; but it does not usually restore that which has been lost. There are other very interesting cases, in which the premonitory symptoms of acute glaucoma exist, and in which the operation, timely performed, prevents the acute attack; but this belongs rather to minute details, upon which I do not propose to enter. —*British Medical Journal, Jan.* 20, 1864, *p.* 19.

71.—ON IRIDECTOMY.

By Haynes Walton, Esq., Surgeon to the Central London Ophthalmic Hospital, &c.

"Iridectomia," or iridectomy, as it is expressed in English, is the term originally introduced to express the operation for the formation of an artificial pupil by removing a portion of the iris. More commonly, however, we say artificial pupil by excision. The modern operation is the same, only on a far more extended scale.

Iridectomy to cure disease was first performed by Professor Von Gräfe of Berlin, and was quickly taken up in England, in consequence of certain circumstances which it is unnecessary to narrate.

Von Gräfe's attention was first drawn to the efficacy of the treatment in internal inflammation of the eye-ball, by observing the results of the operation for artificial pupil by excision after irido-choroiditis with closed pupil. In such cases, he ascertained by experiment "that the increased power of vision was in no way dependent on reabsorption of the pupillary exudations, but was entirely to be ascribed to an improvement in the choroidal complications." I quote from Mr. Windsor's translation in the Sydenham Society's Volume for 1859.

Encouraged by success, he proceeded to operate on cases in which, after previous iritis with pupillary exclusions, there had resulted atrophy of the globe of the eye from secondary choroiditis. He treated by repeated iridectomy many cases in which the globe had become very soft, and much flattened by the action of the recti muscles, and it was ultimately perfectly re-filled. He relates the case of a man to whom he had formerly given a certificate of incurable blindness on account of atrophy of both globes. Vision was limited to a dull quantitative sensation of light. At the time of writing, the operation having been performed *six times*, the patient could count fingers at the distance of some feet, and distinguish letters of the largest print, and the vision was weekly improving.

This statement about resuscitating collapsed eyes puzzled me for a long time; at last I was able to understand it. After an operation by solution for soft cataract, in an unhealthy and softened eye-ball, I found the softness exchanged for more than natural plumpness; and this actually lasted for weeks, so long, indeed, as the preternatural vascularity arising out of the operation remained. The increased vascular action was the secret. This surely points to Von Gräfe's incorrect observation, and false conclusion. And if so, what a basis to theorise on!

After this he directed the new remedy to glaucoma, then to chronic and recurrent iritis, and irido-choroiditis, and found it specially useful in ulceration and infiltration of the cornea, in partial staphyloma of the cornea, and staphylomata of the sclerotic. In acute glaucoma, the results were said to be very striking; the earlier the operation was done the better; but the duration of weeks, or of months, did not exclude complete restoration.

In chronic glaucoma the disease was arrested, and the existing amount of sight was preserved. More rarely the improvement was temporary; atrophy ensued, but the pain did not recur.

Von Gräfe argues against the retina being originally invaded by the disease; he makes the pathology to be derangement of the choroid, hypertrophy of the vitreous humour by endosmosis, and, as a consequence, pressure on the retina. Will any one attempt to reconcile the two opposed actions, the filling of a flabby eye-ball, and the softening of a tense one by the same operation? He removes a portion of the iris, because he supposes that this diaphragm is the source of the aqueous secretion, and that, by reducing it, less fluid is poured out; less pressure is therefore produced; and, accordingly as the operation is done as to time, so is the retina completely preserved from injury, and rescued from complete destruction. I have discussed this, and other theories that have been advanced on the *modus operandi* of iridectomy at length in my book, and urged my objections; but I shall not notice them here, as I find that, in the letters addressed to you, the writers not only do not stand out for theory, but ignore any such support. But I do not like this off-hand method. I require some explanation; because, to my mind, iridectomy, or the cutting off of a portion of eye-tissue to cure a blood-disease, involves a positive absurdity. According to the showing of Von Gräfe, it gives dissimilar results, and it could not cure, as he alleges it does, in affections of such different pathology as those named by him. It is no answer to ask, how does quinine or opium act? This is not a question of the action of drugs. I presume no one ever expects to be able to read vital processes, and to tell the ultimate workings of living machinery; but I do require some show of reason before I adopt a surgical operation which breaks the structure, and disarranges the wonderful mechanism of the glorious and precious eye.

I pass on to consider practically the effects of iridectomy, and to notice the statements of those who believe in its virtue. At once I say, that benefit is to be got, not through any direct effect on the iris, but solely in consequence of the evacuation of the aqueous humour. It is not, then, the iridectomy that does this, but that which takes place as an inevitable step of the operation as a preliminary measure, namely, the "paracentesis corneæ." More than half a century ago, the value of tapping the cornea was clearly made out by Mr. Wardrop, who did more in this country to advance ophthalmic surgery than any man of his time. His writings attest to his great labour, the accuracy of his observation, and the wonderful extent of his investigations. It is with great pleasure that I refer to his practical and philosophical paper "On the Effects of Evacuating the Aqueous Humour in Inflammation of the Eyes" in the Medico-Chirurgical Transactions, vol. iv, p. 145. He was led to try the remedy in consequence of certain morbid changes in the dead eye, from which he drew physiological deductions, and fortunately afterwards verified them by surgical experiment. This paper is a model for the imitation of those who would conduct original investigations in practical surgery. His first trial, very strange indeed to tell, as apposite to the point, was in a case of milkiness of the cornea, distended eye-ball, and acute inflammatory symptoms. He says, "I discharged the aqueous humour by a small incision through the cornea, and had the satisfaction to find that the operation produced not only an alteration in the degree of transparency of the cornea, but also that the pain and all the inflammatory symptoms were removed." After this I find the plan applied to cases of violent inflammation of the eye-ball, in which the cornea was not involved. Again, he points out that in those cases where the practice of evacuating the aqueous humour is judiciously employed, although the operation may create some temporary irritation, yet its good effects will become immediately perceptible, and in most cases will be permanent. The more obvious of these are a more or less considerable improvement in vision, particularly where there is a cloudiness in the anterior chamber; a complete cessation of the sense of fulness of the eyeball, and pain in the head; and in some cases a very remarkable change in the size of the inflamed vessels. It would almost seem as if he were in this controversy when he remarks, that the great and immediate relief of evacuation of the aqueous humour most probably arises chiefly from the sudden removal of *tension*. He wishes it to be understood that the discharge of the aqueous humour is not to be here recommended as the sole remedy in any case of ophthalmia, but is only to be considered as a powerful auxiliary in some, and in others as a sure, and perhaps the only means of preventing a total destruction of the organ. I advise all surgeons who are interested in ophthalmic surgery to study the essay; for this author claims a far greater range of benefit for his patients than does the warmest advocate of iridectomy. He is assured of its potency in all inflammations of the eye-ball; in purulent and gonorrhœal ophthalmia; in corneitis; in mechanical injuries of the eye-ball. Such, it seems to me, is the nature of the remedy that, theoretically speaking, if found useful in any one condition of inflammation of the eye-ball, we can hardly be wrong in generalising, and trying it in all.

Mr. Wadrop's rationale is this. The object is to diminish suddenly the over-distension of the eye-ball; the evacuation of the aqueous humour must fulfil this intention in a more complete manner than can be conceived probable by any means of abstracting blood from its vessels; for, as the ophthalmic artery comes from the skull, little blood can be taken directly from any of its branches, and it would require a great quantity of blood to be drawn from the temples or neighbouring arteries, to make any remarkable change in the quantity of the contents of the eye; or, at least, a change equal to that which would be produced by the discharge of the aqueous humour. From the advantages also which have been universally found to arise from a sudden depletion of blood, in comparison with what can be derived from a slow detraction, considerable benefits might be expected from the practice.

I have the greatest faith in the remedial efficacy of tapping the eye in all inflammatory affections, and even under other states of disease; and I have often found the repetition to tell, where a single operation has given but little or no result; and in very severe cases I have occasionally repeated the proceeding even several consecutive days. Yet I do not exalt the measure beyond the limit the originator modestly claimed for it.

It would not serve my purpose to do more at present than merely to state my general results, for detail. I could not strengthen my argument; and I might be tedious. Again and again I have seen dispelled, by the corneal tapping, the premonitory symptoms of what would be now called glaucoma. I have repeatedly given relief by discharging the aqueous humour, in many stages of ocular disorganisation, when nothing else would relieve. I have seen a small staphyloma of the sclerotic, resulting from a long continued acute inflammation of the eyeball quite pass away, leaving only marks, after a few tappings, by which the vascular action was quelled. The several symptoms occurred after a needle operation for cataract, and long after the cataract was absorbed. Iridectomy was strongly urged in consultation, but I resisted it, for which the patient says he cannot be too thankful. I have the most thorough conviction, that by this system of tapping, combined with general treatment, is to be secured the highest attainable success in inflammatory diseases of the eye-ball. I use the expression, inflammatory diseases, because I wish to avoid the nice distinctions of ophthalmic nomenclature. There is sure to be a difference of opinion among those addressing you on this point; and, indeed, I know no greater ophthalmic puzzle than to point out practically the distinctive differences between what is called acute glaucoma, choroiditis, and arthritic iritis. There is a deal of confusion here for future investigators to simplify. Many of the cases of so-called glaucoma that I have seen operated on, I should describe as choroiditis. Thus I have heard some of these affections regarded by another surgeon as "arthritic posterior internal ophthalmia." Again, I find one of your correspondents of great reputation, defining glaucoma to be "the whole course and several varieties of disease, leading to that last hopeless stage of extinguished sight and disorganized tissues." There can be no more comprehensive description than this; it will take in every ophthalmic inflammation; and still more, this gentleman goes even further, and in his several writings speaks of glaucomatous complications, and the glaucomatous element; conditions, that I, as a man practised in eye-diseases, fail to comprehend.

I am anxious to prevent any young surgeon from supposing that tapping the eye is a perfect panacea. It is not so; neither is any other operation a perfect cure for all idiopathic inflammatory affections. Many cases are met with, that baffle all art and science. I only speak of it in the same terms as its author; as an aid, and to be used along with other means. Nor have we yet discovered any practical surgical measure for curing the bad old-fashioned disease called glaucoma, as understood by me, and by my teachers, Lawrence, Tyrrell, Dalrymple, and expressed by Dr. Mackenzie in his works. According to my experience, it is a very rare disease, each attack of which, each paroxysm, inflicts a definite damage. The eye may be destroyed at once; or it may retain some amount of sight after several periods of renewed activity. Tapping will often relieve; and so will iridectomy as a means of tapping. Pain will be lessened, and perhaps the period of attack shortened. I speak cautiously about the amount of substantial benefit to the eye, because I have so often seen all operations and treatment fail and the eye perish; and in cases where there has been no operation, the spoiling of tissues has been incredibly small. It is not a matter of "cut and you save, don't cut and you destroy." However, our duty is to adopt every likely measure that may do good, while it will not damage.

I cannot help a passing remark on the alleged cause of destruction to the eye in glaucoma; namely, pressure on the retina, of which the greatest evidence is said to be got in "cupping" apart from glaucoma, and even when the eye-ball is not hard; and I am perfectly sure that it is not to be seen in many of the cases of acute glaucoma, when the fundus of the eye can be examined. It more commonly exists in the eyes that have long been blind. There it appears, I suspect that the nerve-tissue has been primarily invaded by disease, or coincidently with the implication of other parts. I regard pressure, if it have any influence at all, as acting secondarily. When this nerve-change exists, by which the eye must be more or less spoiled, I do not believe that any treatment is available. To say that the cupping becomes less, is to admit the generation or reproduction of nerve-tissue. And how could it be proved that the cupping is less? and if it could be demonstrated, would a fractional change be of any advantage, when the least cupping is known to be associated with damaged sight?

The glaucoma, as in every other acute inflammatory ophthalmic disease, the eye-ball is tense, it is hardest of all in long standing glaucoma, and this can only be by an increase of its fluid contents. The choroid and the iris hold very nearly all the blood that is in the eye, and there is an increase in the size, and perhaps even in the number of blood-vessels. It is likely, too, that exudation from the vessels tends to the augmented overfulness. Now all this takes place in every case of severe inflammation of the eyeball, whether it be from purulent ophthalmia, from wounds, from mechanical injuries of all kinds, from gout, rheumatism, syphilis; and who ever found the optic disc cupped from this pressure? It is likely enough that the pressure on the retina has the largest share in eclipsing sight; but after the inflammation from these causes have been subdued, although weeks have elapsed, the eye is often restored to perfect integrity. The same may be said when the eye is pressed on from without by tumours. Sight may be lost for a time; but if the pressure be removed, the patient sees again, and the ophthalmoscope shews a perfect eye. Too much stress has been laid on the tension of the eyeball. When there is the increased volume in the eye from inflammation, tension must of course always exist; and if it be made a signal for iridectomy, I can only say that the surgeon in ophthalmic practice who so regards it, and acts, will always have a large stock of cases ready for his knife, and in proportion as he multiplies his operative surgery, so will he decrease the eye-sight of his patients. It is through such examples that we get the astounding records of wonders. Mrs. A. is blind to-day, as she might indeed be from rheumatic inflammation, &c. She is "iridectomised," and in a week she can, with spectacles, and while looking through a pin-hole, read No. 10 Gräfe's type. This poor woman would have read No. 6 without any appliance, if her iris had not been trimmed, and if general treatment only had been used. I rejoice to think that in private practice I have saved many cases like Mrs. A's from damage.

It will not do to say that there is a difference in the kind of tension between that arising from glaucoma, and that from other inflammations. Moreover, I will not allow that such pressure admits of being recognisable under several degrees—seven it is said. Without weight and measure, how can such estimates be conveyed from one mind to another? A medical man consulted me about impaired vision from some obscure cause; I could detect nothing wrong with the eye-ball. He went to a surgeon of considerable reputation, who pronounced one eye to be like ivory, the other like india-rubber. He (the patient) could not perceive any difference to the touch, nor could I. He consulted another ophthalmic surgeon in a high position, who declared that there was no difference in the tension of the eyes. I mention this to show how differently persons estimate things by the sense of touch.

I can state that, I have never met with any bad result from tapping the eye. The worst thing that ever occurred was a slight prolapse of the iris, and that in a single instance.

Iridectomy is an operation that demands very considerable nicety in execution, and is done better under the influence of chloroform. This is not in itself objectionable; but it has significance in my argument. There are many frequent unavoidable dangers attendant on it, immediate and remote, of which the profession are ignorant, and they have been kept in the back-ground.

It is not always possible to complete the operation according to the prescribed rules, especially the tearing away of the iris to its very root, in consequence of bleeding, by which the parts are concealed from view; and in the attempt to remove the blood by pressure, the only available method, there is much risk of dislocating the lens, or of so injuring it as to cause opacity. When the lens is dislocated, the vitreous humour may be lost. But cataract may follow without this direct injury, as I think, by interruption of the nutrition of the lens through the disturbance set up in the surrounding nervous and vascular tissues. It is not correct to say that the cataract is only a consequence of the glaucoma; as I have seen it, produced quickly after the operation, when the iridectomy has been done because of rheumatic inflammation of the eye. I omit speaking much about the accidental wounding of the lens (not an uncommon event), because that will be attributed to bungling.

Then the eye-ball may suppurate—a disaster more imminent when a great deal of inflammation is present. In the the legitimate making of an artificial pupil, a careful surgeon never operates until the activity of the disease which has closed the pupil has passed away, lest destructive inflammation with its contingencies arise.

The operation may be frustrated by the closing up of the pupil, and the gap newly made. The eye-ball may become atrophied. I should like to know how often the eye-ball has been extirpated, because of the ill results of iridectomy. Imperfect union of the edges of the tunic of the eye through which the incision was made, with the bulging of the new and imperfect tissue, occasionally takes place. Who will dare to gainsay all this? Two or more of these conditions might occur in the same individual. A gentleman with old rheumatic inflammation in both eyes, who saw enough to read the newspaper in a good light without glasses, in the interval of acute attacks, was doubly iridectomised against my advice; but two opinions were against mine, so I was set at nought. In the one eye the pupil closed; in the other, cataract formed. A second iridectomy was done in the first eye. I met the poor fellow accidentally at a friend's house, able only to grope his way about. The cataract was shortly to be extracted.

Iridectomy destroys the integrity of the eye by interfering with refraction, adjustment, and the regulation of light to the eye. I never saw perfect vision after the operation; that is, no person so mutilated could see small things and distant, in different degrees of light, equally well with one of the same age, whose eye might be called normal and healthy. I have been careful to inspect this. A very imperfect idea is given in the reports of cases of the true condition of sight. A person may blunder over this or that sized type, a few inches from the face, with a pair of spectacles in a carefully arranged position as to the light, and yet have very imperfect sight. Several whose cases seemed in print to be all that could be desired I have found to be miserable cripples.

Iridectomy also spoils the appearance of the eye. To lessen this it has been proposed always to operate at the upper part of the organ. But the German propounder of the operation objects to this on the score of difficulty, and great liability to accident.

[We append to this paper the opinion of Mr. WHITE COOPER on this subject.]

Its indiscriminate use is fraught with danger: it is far from a slight operation; serious evils not unfrequently attend or follow its performance; and it should be resorted to with the greatest caution, since I am convinced that many of the cases for which it is recommended are amenable to general treatment. —*British Medical Journal, Jan.* 2, 1864, *p.* 20.

72.—ON IRIDECTOMY.

By WILLIAM BOWMAN, Esq., F.R.S.

[Mr. Walton objects to the operation of iridectomy, requiring to know its *modus operandi* before adopting it.]

We know that in glaucoma *the globe is hard*, and that after iridectomy (and by iridectomy I always mean the operation *as performed for glaucoma, and properly performed*), *the hardness is reduced, and the equilibrium of the circulation restored.* When further research shall have explained *the rationale of the hardness*, we may hope to be nearer than we seem to be at present to the explanation of the *mode* in which iridectomy *reduces the hardness.* Meanwhile the most philosophical course is to apply the remedy for the sake of its proved benefit, rather than to discredit it, or decline to adopt it, because we cannot explain its whole mode of acting.

Several very interesting suggestions have, indeed, been made on this head, though none can hitherto be said to be more than plausible conjectures. Von Gräfe himself, with the candour and scientific spirit which distinguish him, does not, or did not lately, profess to explain it fully; and a view which I myself, at an early period, thought to have some probability, has been ably criticised by Donders, and shewn to be untenable, at all events in the form then given to it. But surely all such discussions as to the *rationale*, highly as they must interest the minds of those who are continually witnessing the actual reduction of tension by the operation, should not weigh for one moment against its performance when a patient whose eye is hard stands in present need of our help. Were Mr. Walton once convinced of the reality of the remedy, he would, I am sure, himself lay aside such an objection from mere motives of humanity.

Mr. Walton argues at length in favour of *paracentesis corneæ*, as doing all for the relief of hardness that iridectomy can do. "At once I say, that benefit is to be got, not through any direct effect on the iris, but solely in consequence of the evacuation of the aqueous humour. It is not, then, the iridectomy that does this; but that which takes place as an inevitable step of the operation, as a preliminary measure—namely, the *paracentesis corneæ.*"

And then we have a long reference to Mr. Wardrop's excellent paper written half a century ago, and a profession of Mr. Walton's "greatest faith in the remedial efficacy of tapping the eye *in all inflammatory affections, and even under other states of disease;* and he has often found the repetition to tell, when a single operation has given but little or no result, and in very severe cases he has occassionally repeated the proceeding even several consecutive days. Yet he does not exalt the measure beyond the limit the originator modestly claimed for it." He goes on in the same vague general phrases: "It would not serve my purpose to do more at present than merely to state general results, for details." "Again and again, I have seen dispelled by the corneal tapping, *the premonitory symptoms of what would now be called glaucoma.*" [Yet he presently takes pains to explain that those symptoms which are really of diagnostic value are, in his opinion, valueless; and that he does not feel able to diagnosticate glaucoma except in its last old-fashioned stage, as I shewed last week.] "I have repeatedly given relief by discharging the aqueous humour in *many stages of ocular disorganisation,* when nothing else" [Was iridectomy tried?] "would relieve." However, he has his reservations. "I am anxious to prevent any young surgeon from supposing that tapping the eye is a *perfect panacea.* It is not so; neither is any other operation *a perfect cure for all idiopathic inflammatory affections.*" He then, at length, does speak definitely of tapping in the disease which he recognises as glaucoma, "the bad old-fashioned disease called glaucoma, as understood by me, and by my teachers, Lawrence, Tyrrell, Dalrymple, and expressed by Mackenzie in his works. According to my experience, it is a very rare disease; each attack of which, each paroxysm, inflcts a definite damage. The eye may be destroyed at once; or it may retain some amount of sight after several periods of renewed activity." Here, I presume, he has in view the inflammatory glaucoma, the acute, and perhaps the subacute, forms. Now, let us carefully mark what is his own experience of tapping in these, where his diagnosis shall not be questioned.

"Tapping will often relieve, and so will iridectomy as a means of tapping." [He does not say that he has himself tried iridectomy in such cases while there was still some sight to be saved.] "Pain will be lessened, and perhaps the period of attack shortened. *I speak cautiously about the amount of substantial benefit to the eye, because I have so often seen all operations* [Iridectomy in his own hands?] "*and treatment fail, and the eye perish.*"

Tapping, in fact, as I understand him, moderates the course and intensity of the disese, but nothing is said of its *arresting* it, of its *permanently relieving the tension* which destroys sight, nothing of any *recovery of sight*, or saving from ultimate disorganisation what he calls in another place "the wonderful mechanism of the glorious and precious eye."

The question here raised may appear to persons unacquainted with the recent literature of glaucoma to have much importance. Certainly, if *one tapping* of the aqueous chamber would *permanently* reduce tension in glaucoma, iridectomy would be at once superseded: though if the tappings to be efficacious are *to be repeated*, iridectomy might still have advantages. But unhappily no such permanent result follows; and I think more ample experience only is needed, *together with a more exact discrimination of the glaucomatous condition*, to convince him that *paracentesis corneæ* is at best but a very transient means of reducing eye-tension in glaucoma. The puncture heals in the course of a few hours. and then the intraocular cause of exalted tension (whatever it be) remaining unredressed, the tension is immediately reproduced. For nothing is more remarkable than the rapidity with which the aqueous humour is secreted, and the orifice being closed, the *whole eye contents* are almost immediately restored to their former successive bulk, and the globe is hard again. If, indeed, *with glaucomatous tension, there coexist more or less active inflammation*, with all its secondary consequences and aggravated risks (acute glaucoma or the higher varieties of the subacute), one or several frequently repeated tappings of the cornea may often serve as a temporising measure to abate the inflammatory symptoms and reduce the severity of the attack; but the hardness will be found to remain, and with the hardness the certainty of eventual blindness at an earlier or later period. Thus iridectomy would still be necessary, while the delay the tappings would involve would probably much diminish the ultimate amount of sight saved.

Mr. Walton, in his work (Surgical Diseases of the Eye, 2nd edit., 1861, chap. xxvi), refers to Von Gräfe's memoirs on *Glaucoma* (translated in the Sydenham Society's volume for 1859, published in 1860, by Mr. Windsor of Manchester, one who perfectly understands the subject), in such a way as to show that he has perused them. Let me also quote from the same most competent authority a passage on this question of tapping the eye in glaucoma, which has apparently escaped Mr. Walton's attention, to show that this practice has been long ago considered, largely tried, and effectively condemned.

"Another year (says Von Gräfe in 1858) has passed since I wrote my essay on *Iridectomy in Glaucoma*, for the third volume of the *Archiv*. I have had many fresh cases, and I have operated on them at most different periods of the disease. So far as was possible, I have kept in view all the cases on which I had previously operated, for although the immediate improvement was clear enough, there was yet the possibility of its not being permanent. To my own observations have been added those of many of my colleagues, and now, when I have again to consider critically the question of treatment, I can at once and with pleasure declare, that I have not the least to retract of what I formerly advanced in favour of iridectomy. Some of the cases, indeed, have ultimately been more favourable than I had expected." (Windsor's Translation, p. 359.) "The uncertainty of the result [of iridectomy] in the later stages of acute glaucoma, especially as regards duration, are so directly opposed to its completeness and durability in the acute period, that the advice *to operate immediately and without hesitation*, cannot be too urgent. I must *most decidedly reject the recommendation to try first paracentesis*,—advice which has been urged from unfounded ideas of caution. Three years' extensive experience has taught me, that the results of paracentesis are, in the infinite majority of cases, temporary only. It is true that when methodically employed, it removes the acute character of the disease, and palliates the symptoms; it does not, however, prevent gradual deterioration of vision. Within from three to four months, this was almost invariably found to be the case, on a careful examination of the field of vision. When with these results we compare those of iridectomy, *there can be no reasonable cause for hesitation.* By such trials of paracentesis, the time is lost for radical treatment; for the degree of recovery essentially depends on the length of time the eye has been already affected, and not simply on the condition of the symptoms. Besides, we may reasonably fear that the repeated disturbances of the ocular circulation—and every change of pressure must be considered as such—may render the case less amenable to a treatment, the value of which has been actually proved. My clinic presents a considerable number of cases, where one eye has been treated by paracentesis in former years, the other by iridectomy. In other cases where the treatment had been commenced with paracentesis, iridectomy was ultimately performed on the same eye, owing to the deterioration of vision, and to our having become acquainted with its beneficial action.

Only a glance at the fate of these different eyes is needed, to induce the abandonment of all other treatment in glaucoma than that of the immediate performance of iridectomy." (*Id.* pp. 363-4.)

It will hardly be suspected that I should not have put the same practice to the test on some of the numerous occasions that have been afforded me of doing so, when at various stages of various forms of *typical glaucoma*, for one reason or another, iridectomy could not be opportunely performed, or would not be at once acceded to by the patient. I may state that I have been *uniformly disappointed* as to any *permanent* result, or any saving of sight by this method, with very few exceptions indeed, the phenomena of which only served to support the general conclusion.

Let it not be supposed, however, that *paracentesis corneæ* is an operation of which the value *in proper cases* is undervalued by me. I esteem highly the debt we owe to its eminent proposer Mr. Wardrop, still happily among us, only I reject it in *typical glaucoma* as a permanent remedy; reserving it for certain cases of the glaucomatous class in which tension arises intercurrently in conditions not originally glaucomatous, and when the cause of tension appears to be so far temporary, that if relieved once, or sometimes even a few times consecutively, by puncture, it ceases to recur; and yet where, if unrelieved by the local outlet, it persists with all its usual ulterior consequences; only yielding to surgical relief. I have particularly found this puncture useful in the tension that sometimes follows needle operations for clearing the pupil after extraction of cataract, and I would earnestly counsel surgeons in any such case, even when the operation has been in itself of the slightest character, to watch narrowly for the occurrence of tension. The pain that is apt to come on from the second to the eighth day often originates in tension, and a timely puncture (perhaps repeated once or twice if necessary), carried also once or twice through the iris if it bulges, will generally allay it in a very satisfactory manner.

I also need hardly say that I employ the useful little operation of tapping the aqueous chamber in some of those cases of *non-glaucomatous* inflammation of the cornea, anterior chamber, and iris, in which Mr. Wadrop recommended it. Here it sometimes, *though there be no actual tension*, sets forward the eye on a course of recovery, where the symptoms had previously been tedious or threatening. Very much might be written on the various conditions here alluded to, were this the place for doing so.

I must add, however, some remarks on the operation of *paracentesis corneæ*, lest it should be thought by any to be a proceeding unattended with danger. It fact, it is often a very delicate and critical proceeding, and it is never free from those risks which belong to every penetration of the aqueous chamber, especially when shallow, by an instrument. If it is to be repeatedly performed, these risks are repeatedly incurred. They have not been disguised by Mr. Wardrop, who says, "The chief difficulty arises from the pain occasioned by the pressure on the eyeball, necessary to keep open the eyelids; for until a sufficient portion of cornea is brought into view, and the movements of the eyeball completely under the management of the operator, the introduction of the knife should not be attempted."

And after describing the mode of holding the eye, and the share "the assistant" must take in the proceedings, he says very truly, that the patient is very apt to start when he finds the instrument coming in contact with his eye. I may add, that he is very apt to start also while the instrument is turned on its axis, and held stationary in the wound (as very properly advised by Mr. Wardrop, as well as by Mr. Walton), in order to favour the escape of the aqueous humour. And when the fluid is thus escaping, and the iris and lens are falling more forwards towards the cornea, the severe pain often felt is very apt to cause instinctive movements of the patient's head or eye, very dangerous to the lens, especially if the cornea be at all cloudy, so that the point of the needle is obscured from view. In glaucoma, also, the anterior chamber is so frequently very shallow, that, if *paracentesis corneæ* be attempted, the risk to the lens is much increased, even on the first entrance of the instrument, especially if the eye be also inflamed, and the patient rendered more acutely alive to the pain by having undergone the same treatment on previous occasions. Mr. Walton himself says of paracentesis (*Surgical Diseases of the Eye*, 2nd edition, 1861), "I believe that this evacuation is seldom practised in the present day, and that for many years it has been discontinued. Mr. Lawrence writes forcibly against it, prefacing his objections with the statement, that he has tried it in some instances, but with so little benefit, that he has not been induced to insist on the practice," p. 48; and again, "Should chloroform not be used, considerable difficulty will be found in exposing and steadying the globe of the eye; *when the patient is insensible*, the operation is very simple," p. 49. Compare this, by the way, with the following objection to iridectomy in Mr. Walton's recent letter: "Iridectomy is an operation that demands very considerable nicety in its execution, and is done better under the influence of chloroform. This is not in itself objectionable, but it has significance in my argument. There are many frequent unavoidable dangers attendant on it" [whether on the use of chloroform or iridectomy is not clear], "immediate and remote, of which the profession are ignorant, and they have been kept in the background." Mr. Walton says of paracentesis: "Common care will save the iris from injury, and the lens will be spared if the points of the instrument be kept forwards," p. 49. It may be said with equal propriety of iridectomy, that common care will save the patient from all the operative risks, provided that the eye is made quiet by chloroform, and the operator is well acquainted with the exact anatomy of the parts he is dealing with. Now, if chloroform is to be given whether paracentesis or iridectomy be performed, most patients would prefer that which has to be done but once, with a prospect of permanent benefit, to that which would probably have to be repeated on several successive occasions, without the surgeon even then being able to promise a real arrest of the disease. And this particularly applies to the subjects of glaucomatous disease, often weakly, nervous, or elderly persons, already depressed by pain and the dread of blindness.

It may not be out of place also to note here from one of the most eminent authorities, Dr. Jacob, a passage relating to the operation of tapping the eye for staphyloma (the eye distended by glaucomatous tension), and which I find in Mr. Walton's work, p. 333: "All you want is to let out all the aqueous humour that will flow, and to leave an opening likely to heal by first intention, until you see how your patient bears injury; mind that, for I can tell you that a very small matter sets up awful inflammation in some people. I have not seen either afford the least benefit, except temporary relief from pain. In a few days or weeks there has been a re-accumulation of fluid, and a return of the symptoms. In one case, after the second tapping, acute inflammation with suppuration followed." Mr. Walton thus quotes Dr. Jacob, *àpropos* of a case, under his own care, of staphylomatous distension of the whole eye, evidently of glaucomatous type. Mr. Walton tried this plan of repeated tappings, with external pressure, and *ultimately performed "abscission."* Probably a timely iridectomy would have allayed pain and saved the blind globe from abscission.

I by no means think, indeed, that one not habituated to operations on the eye can always safely undertake either paracentesis or iridectomy. In itself, no doubt, paracentesis is the simpler. The question, however, is not which is the simpler, but which is the *more effectual for the permanent relief of tension.*

If in the passage above quoted Mr. Walton means that the risks *of the operation of iridectomy* have been kept in the background, I venture to differ from him. I think it is not so. They are well understood, and have been repeatedly stated. It is easy on paper to magnify these risks; no doubt much will depend on the operator's skill and delicacy of hand, and something on the individual peculiarities of the case. I have no wish to underrate the difficulties; rather I would describe them in order to explain how they may best be overcome. I believe they may be, in a very great degree, obviated, or, at least, reduced within very narrow limits. But were they much greater than they are, they would, in most cases, be wisely and cheerfully encountered, under the alternative of impending blindness.

[The following account of the operation of iridectomy, published in 1862, by Mr. Bowman, is very clear and concise. It is quoted by him in answer to some objections by Mr. Walton.]

"The operation is best done when the patient lies on a sofa on his back, with the surgeon standing at his head. I prefer to use chloroform, though I have often operated without it. It should be given so as to render the patient completely passive; for the great delicacy of the operation requires perfect quietude of the eye, lest the steps should not be severally completed in the most perfect way. My own opinion undoubtedly is that there is hardly any person to whom chloroform may not be safely administered; though, it is true, some objects demand more care in its exhibition than others. The sickness may usually be avoided by taking care that no food be in the stomach at the time; and if it occur during the operation, the steps must be simply delayed while it lasts, not varied in any way. If the sickness is very straining, so as to distend the vessels of the head and face, I usually close the eye, and gently compress it by the fingers on the lid, during the efforts at vomiting. I have not seen any harm happen from such vomiting, when the incision has been properly made; not too extensive, and not too far back from the corneal margin.

"I always keep open the lids by the wire speculum, which an assistant holds a little forwards if it tends to exert pressure on the globe. It is well for the surgeon to be able to use the right or left hand indifferently in making the incision, as he can then select the most convenient spot. I have always preferred to make the iridectomy nearly or quite upwards; because I believe this direction to be as good as a lateral one in reference to the visual field, and the upper lid then covers the gap in a way useful both optically and for appearance sake. It is, however, rather more easy, on the whole, to make the iridectomy to one side than upwards.

"I make the incision in one of two methods, according to the size of the anterior chamber. When there is space enough, it is best to use the triangular lancet-shaped blade, inclined at an angle on the flat, and which I believe is used generally abroad, as well as by Von Gräfe. Having selected a place for the incision, I seize the conjunctiva with proper forceps immediately opposite, and thus fix the globe without making any pressure upon it, or pulling it from its bed. The lancet is then thrust in so as to enter the anterior chamber at its rim immediately in front of the attached border of the iris, and is carefully advanced towards the opposite side so as to form an opening of the required size; and if the opening cannot thus be made as large as is desired, it is enlarged at one angle on withdrawing the blade. When, however, the chamber is shallow, I prefer what I at first always used; namely, a narrow extraction knife, running its point along the rim of the chamber for the requisite extent, and making the counter-puncture much as in ordinary extraction. Thus the instrument avoids the pupillary region and the lens. The operation is more difficult where the chamber is shallow. Whichever instrument be employed, it enters a little behind the apparent junction of sclerotica and cornea, in the sclerotica, and in entering the rim of the anterior chamber, it usually passes across that junction and through a very little of the corneal tissue just in front of the pillars of the iris.

"As the instrument used in making the incision is withdrawn, the aqueous escapes; and it is well to let it do so gradually, and to keep the point of the instrument towards the cornea rather than towards the lens. The iris may now be found either to remain in the chamber or to prolapse. If the former, the small slightly curved iris forceps are to be introduced (closed) into the chamber, and made to seize the iris opposite the middle of the incision, about midway between its pupillary and outer border. The iris is then brought outside the chamber and divided with small scissors, on one side of the forceps, from the pupillary to the ciliary border, the forceps pulling it gently at the same time, so as to ensure this complete division of it. The end held by the forceps is then torn from the ciliary attachment as far as the angle of the incision, and even dragged upon a little, so as to detach it beyond the angle, and then divided with the scissors quite close to the angle. The cut end then retreats within the chamber. The opposite side of the prolapsed part is then seized and dealt with exactly in the same manner. No iris should be left in the angle of the incision, lest the healing process be imperfect, and subsequent irritation occur.

"If the iris at once prolapse on the completion of the incision (it is often bulged by aqueous humour of the posterior chamber), the forceps need not be introduced within the incision, but may seize it outside. The less any instrument enters the anterior chamber the better, for fear of damage to the lens.

"If any blood flow into the anterior chamber during the operation, it is as well to allow it to escape before it coagulates. This is best done by inserting a fine scoop within the lips of the incision (not into the chamber), and at the same time by making, if requisite, slight pressure on the eye by the forceps which holds it. The cornea should not be pressed on, lest the lens receive any injury; and, rather than run the slightest risk, the blood may be allowed to remain, as it is very soon dissolved by the aqueous humour, and flows out or is absorbed.

"The operation just described ensures the excision of a complete segment of the iris, from pupillary to ciliary margin, of a width determined by the size of the incision, and which may be usually about a sixth or a seventh of the whole circle."

Wishing as far as possible to elucidate every point on which difficulties are felt, I will notice some few additional objections of Mr. Walton. Such are the following:—"It is not always possible to complete the operation according to the prescribed rules, especially the tearing away of the iris to its very root, in consequence of bleeding, by which the parts are concealed from view; and in the attempt to remove the blood by pressure, the only available method, there is much risk of dislocating the lens, or of so injuring it as to cause opacity.

Answer. See above in my account of the operation. I think the lens could hardly be dislocated or injured, unless the incision be made too far back, or pressure be made rudely or too near the lens-region. It is very rare indeed to find this part injured in an iridectomy properly performed. Cataract also very rarely follows as a consequence of the operation.

Mr. Walton. "But cataract may follow without the direct injury, as I think, by interruption of the nutrition of the lens though the disturbance set up in the surrounding nervous and vascular tissues."

Answer. The disturbance is *already set up* in the surrounding nervous and vascular tissues by the *glaucomatous disease.* The iridectomy by its efficacy in arresting this, relieving pain, and equalising the circulation, must apparently have a direct contrary tendency to that attributed to it here. Cataract may occur in connection with glaucoma, when the iridectomy has been performed, in either of three ways.

1. It may have preceded the glaucoma and the iridectomy, the glaucoma having come on during the progress of the cataract; of this I have seen several instances. In these cases the glaucomatous complication has first to be dealt with by iridectomy, and at a subsequent stage, when the tension has been reduced, and the usual indications for cataract operation have arisen, the cataract may be dealt with. In such cases I have never seen the operation of iridectomy hasten the progress of cataract. (See Ophthalmic Hospital Reports, vol. iv, p. 51).

2. The cataract may be glaucomatous cataract, *i.e.*, that sort of opacity of the lens which usually takes place at an advanced stage of glaucoma, when the nutrition of all the ocular tissues has been interfered with through long continuance or intensity of tension, and the disturbance of circulation thereto pertaining. It is rare for this form of cataract to occur while any sight remains worth attempting to save by an iridectomy. But it must be borne in mind that iridectomy by its power of relieving tension may often render great service to an eye already blind from glaucoma, by reducing its inflammation and putting a period to the protracted sufferings of which it is too often the seat, and which are proof against other means short of excision or the seton. Here, of course, Mr. Walton's objection does not apply.

3. A cataract might arise as a direct consequence of the operation. This might be by direct wound of the lens, which certainly ought not to happen; by bruising or otherwise injuring it, without direct contact of the instrument; this also ought not to happen, and if it did so, must not be charged to the operation, but to the operator. While the steps of the operation were less familiar to us than now, and its liabilities had been less studied, this happened sometimes; once to myself; but now it could hardly occur; it ought not to occur.

Von Gräfe has seen on a very few occasions the formation of cataract after iridectomy when the lens had not been touched, and he was led to attribute it to a bulging of the equator of the lens on the side of the iridectomy, with a rupture of its capsule into the canal of Petit. Such an occurrence must be regarded as most rare and as having no force as an objection to iridectomy, performed as it is for averting blindness. Besides, when the glaucomatous state is arrested, a cataract may be operated on at a proper period with good prospect of recovery, as I have myself proved in several instances; so that even if unhappily cataract should by any accident follow the operation in the way of consequence, it is not an irremediable calamity, though of course to be deplored. But what is the alternative? That all glaucomatous patients are to be deprived of the benefits of iridectomy, or be abandoned to the method of repeated tappings with its eventual resulting blindness?

Mr. Walton. "Then the eyeball may suppurate—a disaster more imminent when a great deal of inflammation is present."

Answer. Suppuration, except as an incident of the last disorganised stage of glaucoma, is extremely rare. The inflammation of glaucoma has not a suppurative tendency. It is more allied to the serous and hemorrhagic inflammations of other parts. At all events so rare is suppuration, that I hardly remember more than one or perhaps two examples of it, and in one of these the phlegmonous form of inflammation supervened upon the serous after a paracentesis of the vitreous humour, which for several hours both gave marked relief to tension and to pain, and also improved vision. An eyeball sometimes suppurates after the most trifling operation on its interior, but Mr. Walton would not therefore refrain from such interference when it was reasonably indicated. I think, undoubtedly, suppuration is more rare after glaucoma-iridectomy than after extraction of cataract. I do not, however, here include cases in which it is performed on old blind painful glaucomatous globes, not for recovering sight, but for ease. Then suppuration may sometimes prove a happy event; or atrophy without suppuration, which is also recounted by Mr. Walton among the objections.

Mr. Walton. "The operation may be frustrated by the closing up of the pupil, and the gap newly made." This may happen in the glaucomatous stage of iritis or irido-choroiditis; or when iridectomy is undertaken under certain circumstances (as for recurrent iritis, &c.,) without undue tension; and it must then often be accepted as an intermediate condition on the way towards ultimate recovery of sight. But this objection does not correctly apply at all to iridectomy performed for typical forms of glaucoma, where the pupil is certainly left large enough.

Mr. Walton. "Imperfect union of the edges of the tunic of the eye through which the incision was made, with the bulging of the new and imperfect tissue, occasionally takes place." This is what Von Gräfe has recently described at length, with many interesting particulars, under the term *cystoid cicatrisation* (see Ophthalmic Hospital Reports, vol. iv, p. 53.) To the translation of his paper, I have added some remarks embracing my own experience on this subject up to last October, and might add here some additional particulars, but that it would carry me too far. Von Gräfe has met with this in a greater or less degree in about one-fifth of his cases. It is generally harmless, sometimes perhaps even salutary, by affording a yielding point at which any remaining tendency to eye-tension may so far find relief as to save the eye from its injurious effects. In some very rare cases it may perhaps prove a source of eventual trouble. There may be found means of disarming the operation of iridectomy of this liability to this cystoid scar.

This and many other subordinate questions are now engaging attention, but appear to me to form no argument at all against an operation which constitutes the only remedy yet known for typical glaucoma.

Mr. Walton. "I should like to know how often the eyeball has been extirpated because of the ill-results of iridectomy?"

Answer. I cannot tell. The question might have been asked by an opponent of the operation of placing a ligature on one of the great arteries, when that seemed the only rational and scientific method of treating a disease threatening life, 'I should like to know how often has a limb or a life been sacrificed because of the ill-results of this tying of the great arteries?' The severity of the disease and the infinite variety of individual conditions under which it occurs will cause some among a multitude of cases to have an unfortunate result. Every medical man will understand this from his experience of any single disease, especially if it be one most apt to occur in the feeble and the aged. But of all the operations for the sinking or removal of an eye that has become blind from glaucoma (while some have had iridectomy previously performed, almost always for the relief of pain, not for the restoration of sight, which was already lost), the large majority had certainly fallen under that sad necessity *through the want of a timely performance of iridectomy.*

One further objection of altogether a different nature has still to be met. Mr. Walton calls iridectomy "an operation which breaks the structure and disarranges the wonderful mechanism of the glorious and precious eye." "Iridectomy," he says, "destroys the integrity of the eye by interfering with refraction, adjustment, and the regulation of light to the eye. I never saw perfect vision after the operation; that is, no person so mutilated could see small things and distant, in different degrees of light, equally well with one of the same age, whose eye might be called normal and healthy. I have been careful to inspect this. A person may blunder over this or that sized type, a few inches from the face, with a pair of spectacles in a carefully arranged position as to the light, and yet have very imperfect sight. Several whose cases seemed in print to be all that could be desired, I have found to be miserable cripples."

Answer. An amputation necessary to save life cannot rightly be called a "mutilation"; or, if it be so, it is a mutilation of which science and humanity may boast. If iridectomy be proved necessary to save sight—even partial and imperfect sight—the patient may well be grateful for *an eye which sees,* thought it have been rescued from blindness at some sacrifice of its physiological perfection. Mr. Walton says he has been careful to inspect the results of iridectomy on vision. He has at least declined to inspect cases which I freely offered to show him; and an inspection of which would have rendered my explanation on this point, and probably my whole reply, unnecessary. The fact is, that an iridectomy, especially if performed upwards, leaves the eye but very little the worse as an optical instrument. It is only disadvantageous by in some degree uncovering the marginal part of the lens and admitting some rays which do not form so perfect a focus on the retina as those which still pass through its centre. The act of adjustment or accommodation, as it depends on the ciliary muscle, is not affected by it; and if it were, the subjects of operation being generally of an age when presbyopic glasses are necessary for near vision, the variation in the power of the convex glass necessary can be of no importance. And, indeed, I have often seen a patient after iridectomy read with a weaker glass than before, owing, apparently, to the glaucomatous disease often presenting hypermetropia or increased presbyopia, as one of its conditions, as long ago pointed out and explained by Von Gräfe, and to the iridectomy having lessened this.

But, after all, an exact examination of the state of vision after a successful iridectomy, where the retinal function has been preserved entire, and therefore where the *optical* state may be separately scrutinised, often clearly proves that the iridectomy leaves the eye with the power of seeing distant objects perfectly, according to the most searching tests, and also (with glasses suited to the time of life) able to read the smallest type. I venture, therefore, to say expressly, that Mr. Walton, in raising this objection, shows himself to have had as yet too few opportunities of examining the real results of the operation, where it has been suitably performed.

The last objection is, that "iridectomy also spoils the appearance of the eye." *Answer.* If the operation be performed upwards, the pupil retains its natural aspect, being partly covered by the upper lid, and if performed inwards, which is the other direction usually followed, its new shape is hardly visible to an ordinary observer. I never heard an objection on this score from any patient.

In conclusion, let me add that as, in this and the preceding letters, the subjects of glaucoma and iridectomy have been treated in the necessarily partial aspects incidental to a controversial discussion, and of course with no pretension to be a complete account of the modern doctrine and treatment, I desire my paper of August 6th, 1862, published in the British Medical Journal of October 11th of that year, to be regarded as explaining and supplementing whatever may have been here incompletely expressed.—*British Medical Journal, Jan.* 30, *and Feb.* 6, 1864, *pp.* 133, 165.

73.—ON IRIDECTOMY.

By S. Browne, Esq., L.K.Q.C.P., Belfast.

[After devoting considerable time and attention to the subject, Mr. Browne has come to the conclusion, that the *exact* nature of the disease of the eye named "glaucoma" by the iridectomists, has not yet been accurately defined, nor has the operation for its cure been based upon the recognised principles of scientific surgery.]

My individual experience of glaucoma and iridectomy may be summed up very briefly. During the last four years, I have carefully observed every case of eye-disease, more than five thousand in number, that has come before me; and I have not been able to recognise more than one per cent. which I could classify as glaucoma; namely, deep-seated inflammation of the tissues of the eye-ball; dilated pupil; probable effusion within its structure; and tension of the entire globe; severe pain, with impairment or almost complete loss of sight; the disease at the same time running a more or less rapidly destructive course, and not being controllable by medical treatment. But I have seen, within the same period, a large number of cases of acute inflammation of the deep tissues of the eye—with the symptoms of effusion; namely, dimness of vision, in several degrees—and appreciable tension of the globe. These were cases of choroiditis, or chronic iritis; were perfectly manageable; and in them, under proper treatment, the eyes were restored either to tolerably good or to unimpaired sight.

In some of the cases referred to as glaucomatous, I performed, with all the care I could exercise, the operation of removing a portion of the iris, amounting to about one-sixth of its bulk; and although by the step I have alleviated, in some instances only temporaily, the sufferings of the patient, I regret that I have not in any case been able to restore the sight to a more perfect condition than has followed after constitutional treatment and the evacuation of the aqueous humour; and that, too, in cases with analogous symptoms.

By these remarks it will be seen that, in this country, acute or chronic glaucoma is a disease fortunately of rare occurrence; and that my personal experience of iridectomy for its treatment has not created in me any sanguine hopes of its efficacy. But here I must guard myself from being misunderstood. I simply refer to the operation of iridectomy as applicable to the treatment of the so-called glaucomatous affections of the eye; as, in common with every ophthalmic surgeon, I have found the formation of an artificial pupil by incision and removal of a portion of the iris—in fact, by iridectomy—in suitable cases, frequently successful in restoring very useful sight. I may likewise remark, that I have found iridectomy a good operation in cases of cataract with synechia posterior, which I have extracted by linear section.

My own experience of iridectomy in the treatment of glaucoma having only afforded me negative results, I naturally felt anxious to read the correspondence which has appeared, hoping that I might receive important information, and possibly have my views either changed or modified; but, alas! after all the ink that has been shed in the controversy, I find that I am not much better informed upon the subject; and that I cannot consistently alter the opinions which I had already formed, founded upon my personal observations and on my independent judgment. And certainly I now feel myself not only justified in thus holding by my former views, but also in publishing them before the profession; for I maintain that it is the bounden duty of every man, who practices general or special surgery, to investigate the principles of every novel proposition touching his art, and to adopt or reject it manfully, according as his reason and experience may guide him.

I have already stated my reasons, based on personal observation, for rejecting iridectomy as applicable to the treatment—I dislike the term "cure", it is not nicely professional—to the treatment of glaucoma; and I shall now briefly state why it is that I still reject it, even after the lengthened, I cannot say learned, correspondence which has taken place regarding the operation.

In the first plece, then, I find that its advocates are at variance relative to the pathology of glaucoma. Several of them, indeed, appear to ignore every pathological consideration; and there are others who really do not seem to comprehend the matter under discussion, though strongly advocating the operation of iridectomy as the "cure" in certain undefined conditions of the eye; while, at the same time, they forget every principle of surgery in their haste to vote upon the question!

The second consideration suggested by the correspondence, is the vast numbers of cases of the so-called glaucoma, that appear "as thick as leaves in Vallombrosa" in and about London! Well, not having seen them, I cannot say that such did not and do not exist; but I may be allowed to entertain a shrewd suspicion that many of the cases which on this side the Channel we regard as simple though severe inflammation of the deep structures of the eye, are pressed into the service of the iridectomists, the admirers of the "new" and "brilliant" operation. Otherwise, how is it that, while we have here the usual amount of cases of choroidal, corneal, and iritic disease, we see so very few of those dire affections that can only be cut short by cutting up the iris? I maintain that we have the usual proportion of severe inflammation of the deep structures of the eye, and that these are quite amenable to treatment without iridectomy; and I likewise maintain, after more than twenty years' experience in an ophthalmic institution and in a general hospital, that I have not seen two per cent. of acute or chronic glaucoma, as I understand that disease, which I have already defined—cases, in fact, which were not controllable and controlled by suitable medical and surgical interference; iridectomy not forming a part of the treatment.

Then, as regards "tension" of the eye-ball, which *now* seems to be the great point, the one index to guide the ophthalmic surgeon in selecting the cases for iridectomy, I unhesitatingly affirm that, if any surgeon will presume to operate upon every eye in which there is failing vision, and in which he thinks he discerns undue tension of the globe, he will take a most unjustifiable step, and will act in violation of sound pathological principles. And I say this the more readily, as the iridectomists have not failed to affirm that the opponents of the operation are doing a vast amount of wrong in not adopting it. But why do I say so? Simply because I have, over and over again, treated cases, and have under my observation at this moment patients, where the eyes, suffering from acute choroidal, corneal, and iritic inflammation, have or had the symptoms that have been assigned to glaucoma, failing sight and tension of the eye-ball being prominent signs; and yet, by proper treatment, iridectomy not forming any part of that treatment, the eyes have recovered, or are recovering, with useful or good sight; and that is as much as the iridectomists can claim for their most brilliant operation! Nay more, I have lately had a patient with goître under my care, in whom the eyes were prominent and *tense*, with impaired powers of sight; yet just as the enlarged gland diminished in volume, so did the abnormal condition of the eyes recede also. Would any man in his senses, and with a just regard to the honour of our noble profession, operate in such a case because he had discovered that dimness of sight and tension of the eye-ball were present.—*British Medical Journal, Jan.* 30, 1864, *p.* 136.

74.—ON IRIDECTOMY.

By JABEZ HOGG, Esq., Assistant Surgeon to the Royal Westminster Ophthalmic Hospital.

[The following is Mr. Hogg's opinion of the utility of the operation of iridectomy, and it explains the practice adopted by the other surgeons to the Westminster Ophthalmic Hospital.]

I do not *altogether reject iridectomy*; but, on the contrary, I —I may say *we*—employ it whenever I think that by so doing I can benefit my patient. It is true, the casual visitor to our hospital may not have seen this operation resorted to so frequently as at some other institutions for the treatment of eye-diseases; but this in part arises, or apparently so to myself, from the glaucomatous disease falling less frequently under our observation and care than it would appear to do elsewhere; and also from another circumstance, that if I see the case at the onset of the attack—in the acute stage, when it is nearly always accompanied with pain and increased tension of the eye-ball—I confess I have so much confidence in the simpler and safer operation of my colleague Mr. Hancock, that I immediately resort to section of the ciliary muscle; and this is generally attended with so much relief to the more urgent symptoms, that the after-treatment becomes a matter of little or no anxiety as to the ultimate condition of vision. But, in the more advanced stages of this disorder, I have no hesitation whatever in saying, so far as I have been able to form an opinion, that iridectomy appears to me to be the operation which holds out the best chance for the patient.

I may add that, as a rule, I more frequently resort to iridectomy in *irido-choroiditis;* for the relief of which, indeed Desmarres, the originator of the operation, so successfully employed it.* This affection appears to ensue as a consequence of the inflammatory action extending from the choroid to the iris, and ultimately to the retina. Such cases, in the early stage, very much resemble glaucoma, and quickly run on to closure of the pupil or total destruction of the vision. It is, therefore, incumbent upon us to make a free iridectomy. The details of Desmarres's method of performing this operation will be found at page 263, last edition of my Manual of Ophthalmoscopic Surgery. —*British Medical Journal. Jan.* 23, 1864, *p.* 108.

75.—SUB-ACUTE GLAUCOMA OF BOTH EYES—TREATMENT BY IRIDECTOMY.

By J. G. Hildige, Esq., Dublin.

[The patient was an unmarried lady of the age of thirty-five. Ten days previously to consulting Mr. Hildige, she had noticed for the first time, that the vision of the left eye was indistinct; after four or five days intense pain set in in the eyeball and temple, followed almost immediately by partial loss of vision.]

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* Von Grafe has been so constantly referred to as the originator of iridectomy, that it is well I should remind the profession that the eminent French oculist Desmarres devised and practised the operation for the relief of *irido-choroiditis*, long before the first-named surgeon took it up.

On examination, I found the eye in the following state:—Eyeball of stony hardness, slightly prominent, and painful on pressure; conjunctiva injected, particularly in the neighbourhood of the ciliary ligament; ciliary vessels prominent and tortuous; pupil dilated; iris discoloured, of a greenish hue, and arched forward so as almost to obliterate anterior chamber; the lens and its capsule were quite transparent, but the vitreous humour was so clouded as to prevent ophthalmoscopic examination. She had lost all useful vision with the eye, being unable to recognize the features of persons standing close to her, or to distinguish a single letter of No. 20 of Jäger's test-type, letters about one inch long, when held close to her face. The pain in the eye-ball and temple was continuous, and almost intolerable. The right eye was commencing to show symptoms of the disease also, but as yet there had not been any pain in it, and the sight of it was still good. I performed iridectomy on the left eye, on the following morning, her sight having been tested immediately before the operation by Mr. Colles, President of the Royal College of Surgeons, Ireland, and found to be in much about the same state as on the previous day. On visiting her in the evening, she expressed her satisfaction at the complete relief from pain resulting from the operation. During the night intense pain set in in the other eye, and on the following day this eye also was found to be attacked by glaucoma in its subacute form. Iridectomy was immediately performed, with almost instantaneous relief; no irritation following either operation, which, in both instances, was done upwards, the slight irregularity of the pupils being completely covered by the eyelids. Vision of both eyes gradually improved from this date up to December 16, on which day she called on me at my request for the purpose of having her sight tested in the presence of Mr. Colles, and other professional friends whom I had invited to be present at her examination. Both eyes had recovered their normal tension, no injection of conjunctiva, pupils of normal size, and the irides had assumed their natural colour (grey). The left lens was slightly hazy towards its external margin; the right was perfectly transparent. Both optic discs presented appearances of congestion, and were difficult to distinguish from the surrounding parts of the retinal field. Vessels scarcely discernible. Vision was now so good that she could count the panes of glass in a window at the distance of between thirty and forty yards with the naked eye; she read No. 8 of Jager's test type also without glasses, and with No. 2½ cataract glasses she read small type with each eye separately, to the amazement of the byestanders.

I have not the slightest doubt on my mind that, had iridectomy been delayed but a few days longer in this case, both eyes would have been irretrievably destroyed.—*Medical Times and Gazette, January 2, 1864, p. 8.*

76.—ON DIVISION OF THE CILIARY MUSCLE.

By Henry Hancock, Esq., Surgeon to the Royal Westminster Ophthalmic Hospital.

It was my intention to have related the annexed cases, showing the result of the operation of "division of the ciliary muscle" in certain diseases of the eye, without any observations, had it not been for statements which have been made in the Dublin Quarterly Journal for February, p. 112, and which, if uncontradicted, would have the effect of depriving me of the credit of originating that operation. It is quite true that similar assertions have been made elsewhere, but they have emanated from those who, on the one hand, have strenuously opposed the operation without giving it a single trial; and, on the other hand, from those who, having been thwarted in their endeavours to appropriate the credit of its introduction to themselves, consider it in accordance with the dictates of truth and honesty to turn round and condemn it altogether, and declare that it is not original—that is merely a revival of the exploded method of "paracentesis" of the eyeball. I have hitherto passed by the attacks of such opponents as unworthy of notice; but when a gentleman comes forward and candidly acknowledges that he had been inclined to give the operation his most strenuous opposition, and to laugh at the idea that simple division of the ciliary muscle could effect any benefit in such diseases as glaucoma and irido-choroiditis, but that having tried it in several cases, he has found the results so satisfactory that he is determined to give the operation a full trial; when, I repeat, this gentleman goes on to assert that, after all, this operation is not original, and endeavours to support this assertion by reference to an author of the highest authority, the case is altogether different, and I now unhesitatingly offer a few observations to controvert these statements, and to prove how incontestably erroneous they are.

Referring to the operation of "section of the ciliary muscle," the Reviewer of Mr. Hogg's work, on "Ophthalmoscopic Surgery," in the Dublin Quarterly Journal, observes: "A very short time ago we would have taken very strong ground indeed in advocating the advantage of iridectomy in either glaucoma or irido-choroiditis, and, perhaps, a smile of considerable doubt might have been seen playing about the corners of our mouth when speaking of "section of the ciliary structures" as a substitute for it; but we have lately had opportunities of testing both operations, and our results, from simply plunging a narrow-bladed knife into the eye through the ciliary structures, have been so satisfactory, that we feel determined for the future to give full trial to this operation before having recourse to the more serious proceeding of iridectomy." He continues: "Division of the ciliary muscle and ligament is, however, no new operation. . . . The old paracentesis oculi was simply a division of the ciliary structures, and all the advantages which the older ophthalmologists derived from paracentesis were, we believe, deducible from the fact, that in performing that operation they divided the ciliary structures, as can be seen by the following directions for its performance, taken from Mackenzie. . . . The puncture should be made with a broad iris knife at the usual place of entering the needle in the operation of couching. The instrument should be pushed towards the centre of the vitreous humour, turned a little on its axis, and held for a minute or two in the same position, so that the fluid may be allowed to escape; . . . thus, of necessity, dividing the ciliary structures."

I thank the reviewer for this frank declaration of his experience, but I must be excused for dissenting *in toto* from his propositions. I assert that "section of the ciliary muscle" *is* a new operation, and that it had never been practised as a curative method in diseases of the eye before I introduced it to the notice of the profession.

I deny that "section of the ciliary muscle" and the "old paracentesis oculi" are identical; and I also deny, not only the *necessity*, but the *possibility*, of dividing the ciliary structures in "paracentesis oculi," performed as directed by Mackenzie. The reviewer ought at least to have stated with something like precision the spot indicated by Mackenzie for making the puncture, and the reasons which that gentleman assigned for recommending it; but this he does not do. The puncture we are told, "should be made at the usual place for entering the needle in the operation of couching;" but whether it is to be made at a twelfth, an eighth, a sixth, a fourth part of an inch, or at any other distance from the cornea, is left to the imagination of the reader. In the absence, therefore, of this information, I would refer to the fourth edition of Mackenzie's work, p. 779, where, describing the operation of couching, he says: "The needle is to be entered at the distance of *one-sixth* of an inch behind the temporal edge of the cornea. If this rule is not attended to, but the instrument is entered either much nearer to the cornea or much farther from it, the *ciliary processes* in the one case, and in the other the retina, can scarcely escape being injured." Whilst antecedently, at page 778, he states: "The parts which must be wounded (in couching) are the conjunctiva, sclerotica, choroid, and vitreous humour. The parts to be *avoided* are, the *ciliary processes*, the retina," &c. Mackenzie also points out that the needle should be directed towards the centre of the vitreous. So that, whilst paracentesis oculi and the first steps of couching are the same, it is very evident that division of the ciliary structures is one of the principal points to be guarded against in both these operations. And I would ask how the ciliary muscle and processes can be divided by entering a broad iris knife one-sixth of an inch from the cornea, and directing its point towards the centre of the vitreous humour? It is anatomically impossible. Moreover, the non-identity of the two proceedings is still further illustrated by their results; since Mackenzie (page 899 of the same edition), after describing the operation of puncturing the sclerotic, adds: "A *transient* amelioration of vision, as well as relief from pain, is *sometimes* the result of the operation." Whereas experience has proved the results of division of the ciliary muscle to be lasting, patients upon whom I operated nearly four years ago being still in the enjoyment of their recovered sight.—*Lancet, March* 19, 1864, *p.* 321.

77.—ON OBSTRUCTION OF THE EUSTACHIAN TUBES, WITH A DESCRIPTION OF POLITZER'S NEW METHOD OF TREATMENT.

By Dr. T. M'Call Anderson, Glasgow.

[The use of the eustachian tubes is not only to allow access of air into the cavity of the tympanum, but to permit escape of mucus from it. If the mucus does not escape, as happens when an obstruction occurs, it accumulates, fills up the cavity of the tympanum, and thereby leads to very serious injury of that organ. Dr. Anderson does not wish to enter into the controversy as to whether the tube is always or only occasionally open, but contents himself with stating one or two facts which prove incontestibly to his mind that it is only occasionally open.]

If you blow air through your nose by means of Politzer's instrument, which I shall fully describe afterwards, and at the same moment perform the act of swallowing, you will at once experience a sense of fulness in the ears which does not disappear until you repeat the act of swallowing two or three times; but, if you blow air through your nose when you are not swallowing, no air enters the eustachian tubes, because the levator and tensor palati muscles have not been called into play and the faucial orifice remains closed.

This may be well illustrated if you get a patient who has lost the drum of one ear, and whose eustachian tube is pervious. If you perform this experiment while he is swallowing, air at once rushes out of the ear with a loud whistling noise, but if he does not swallow during it, no air comes out of the ear at all, because the levator and tensor palati are quiescent, and the faucial orifice is not open.

Again, if you expire forcibly, and at the same time keep the nose closed, after the manner of Valsalva's experiment, which I shall explain more fully hereafter, you will perceive the same fulness in the ears as in the previous experiment, owing to an excess of air being driven into the cavity of the tympanum; and this fulness does not disappear till the act of swallowing is repeated, by which means the faucial orifice of the eustachian tubes is opened, and the excess of air allowed to escape.

And if the membrana tympani is carefully examined with a good speculum during either of these experiments, it will frequently be observed that, at the moment the fulness is experienced in the ears, the drum is pushed outwards and remains in that position until the act of swallowing is repeated two or three times, when it retreats to the position which it occupied before the experiment.

Lastly, the effect on the ears of descending in a diving bell has been cited by Toynbee as proving the same thing. "It is well known," says he, "that during the descent the compressed air filling the external meatus produces a sensation of weight, and often of pain, by pressing the membrana tympani inwards. This sensation can, however, be at once eased by an act of swallowing, whereby the condensed air is allowed to enter the tympanum through the eustachian tubes, and thus afford support to the inner surface of the membrane."

These three instances, then, prove that the faucial orifices of the eustachian tubes are not constantly open, but only when the levator and tensor palati muscles are called into play.

But how are we to ascertain that the eustachian tubes are pervious, or rather what are the *essential* symptoms of their obstruction?

In all cases there is a certain amount of deafness, which is very considerable if the obstruction be complete, but of course the exact amount of the deafness, even in that case, will be regulated to a considerable extent by the morbid condition which has closed the tube. Thus, let us suppose two cases, in both of which there is complete obstruction, but in the one this is due to thickening of the mucous membrane at the faucial orifice only, while in the other, it is caused by an old chronic inflammation of the cavity of the tympanum, which has produced adhesions between, and rigidity of, the ossicles—we must expect to meet with a greater degree of deafness in the latter than in the former. But what we have specially to bear in mind just now is, that one of the essential results of closure of the tube, no matter from what cause it arises, is a certain amount of deafness.

Pain, though a common, is by no means a necessary symptom in uncomplicated cases, but tinnitus is almost always present to a greater or less extent. Then the appearance of the drum often affords us valuable information. It has fallen inwards towards the posterior wall of the cavity of the tympanum, for the reason which I previously stated, so that it is much more concave externally than it ought to be, so much so in some cases, that the stapes can be distinctly seen. In consequence of this altered position of the drum, the head of the malleus projects very prominently forwards, so as to catch the eye whenever the speculum is introduced, while its handle is drawn backwards, and appears much thinner and smaller than when in its natural situation; and in extreme cases it is hardly visible at all. Then the surface of the drum is dull, glassy-looking, and irregular, owing to its altered position; and, instead of one triangular bright spot passing downwards and forwards from the point of the malleus, we often observe two or three of varying shapes and sizes on different parts of its surface. Sometimes we find a milkiness or dense opacity of the drum, but only in cases due to, or complicated with, disease of its coats, or of the cavity of the tympanum.

Toynbee describes an ingenious method of ascertaining if the eustachian tubes are pervious or not. "It has already been shown," he remarks, "that during the act of deglutition, with the mouth and nose closed, a small quantity of air is passed through the eustachian tubes into the tympanic cavities; a process which is attended with a sensation of fulness in the ears. The entrance of air into the tympanum can be distinctly heard by means of an elastic tube about eighteen inches long, each end of which is tipped with ivory or ebony; an instrument which I have named the Otoscope. One end of it is to be inserted into the ear of the patient, and the other into that of the medical man, who must take care that no portion of the tube touches any neighbouring body. When the patient swallows a little saliva, the mouth and nose being closed, if the eustachian tube be pervious, at the moment that he feels a sensation of fulness in the ear, the surgeon will hear most distinctly a faint crackling sound, produced apparently by a slight movement of the membrana tympani. This crackling sound is that most usually heard; but in some instances where the mucous membrane of the tympanum is thick, a gentle flapping sound will be detected in its place."

Then there is Valsalva's method, which consists in causing the patient to propel air into the cavity of the tympanum by making an effort at forcible expiration, the mouth and nose being closed. Many patients do not understand what they are meant to do if you give them directions such as the above, and they are much more likely to comprehend and act upon your request if you say—"Blow your nose, but at the same time hold your nose tightly, so that no air can escape." While he is carrying out your instructions, you must watch him carefully, and you soon come to know whether he has understood your meaning; and after you are satisfied of this, put your stethoscope against his ear, or make use of Toynbee's otoscope, and tell him to repeat the operation, but without moving his head in the least. If the eustachian tubes are quite pervious a peculiar "thug," as it is called, or sound like that produced by inflating a minute bladder, is elicited, which is audible both to patient and physician. In most cases there is a certain amount of sound produced, even where the eustachian tubes are almost completely impervious, especially if the obstruction is at the tympanic orifice, thus permitting the transit of air through the greater portion of the tube, so that the physician requires carefully to educate his ear in order that he may be able to discriminate the sound which indicates complete patency—a sound which no mere verbal description can teach. The patient, if the tubes are pervious, not only hears a sound, but also experiences a distinct fulness and disagreeable feeling in the ears, which, however, disappears when he swallows two or three times, so as to open the faucial orifices and allow the escape of the compressed air.

Toynbee's diagnostic test is superior to Valsalva's, in as far as every patient understands what he has to do, and is able to do it; while it is decidedly inferior to it in as far as a very small current of air is propelled with very little momentum into the cavity of the tympanum, in consequence of which the sensation of fulness in the ear is not nearly so distinctly felt by the patient, and the crackling sound not nearly so distinctly heard by the physician.

If you are not satisfied with the information conveyed by means of Toynbee's or Valsalva's experiments, you may have recourse to the eustachian catheter, the use of which has been so fully described in standard works that it is not my intention to occupy your time this evening by discussing it; but I quite agree with Toynbee in thinking that, as a means of diagnosis, it is very rarely required.

Let me now allude shortly to some of the more usual morbid conditions which are accompanied by a temporary or permanent, partial or complete, closure of the eustachian tubes; in doing which I shall touch upon the indications of treatment merely, and shall omit for the present all mention of the mechanical treatment, to which I shall subsequently refer.

Occlusion of the faucial orifice is very often met with in delicate children, especially after fevers or debilitating diseases. In them the tonsils are frequently enlarged, and the mucous membrane of the throat, nostrils, and eustachian tubes hypertrophied, and secreting abundantly. So great, in some cases, is the thickening of the lining membrane of the nostrils, that the patient requires to breathe almost entirely through the mouth; and when, in addition, the tonsils are very large, he is not only apt to snore while asleep, but likewise to speak with a nasal twang. Occasionally, during the act of blowing the nose, swallowing, coughing, sneezing, or any other act which calls the levator and tensor palati muscles powerfully into action, the patient experiences a loud crack in the ear, followed by a sudden improvement in hearing, which lasts from a minute or two to one or two days, and gradually subsides. The amount of improvement at these times depends upon the amount of the previous deafness, and upon whether the obstruction has been, for the time, completely or only partially removed. The oftener this occurs, and the longer the improvement in hearing continues after each crack, the more hopeful must we be of the ultimate success of our treatment, especially if the patient did not experience it till after the treatment was commenced.

Adults are subject, though not nearly to the same extent as children, to a similar obstruction of the eustachian tubes from like causes, but much more frequently in connection with with acute or chronic coryza, relaxed sore throat, or bronchitis. The tonsils are not nearly so often in a state of hypertrophy.

The treatment in children consists in a careful regulation of the diet, attention to the state of the stomach and bowels, and the internal administration of cod-liver oil and tonics, especially those containing iron. Exercise in the open air is to be recommended, and a cold sponge bath in a morning, the patient being afterwards rubbed with a flesh brush or rough towel till thorough reaction is established, else harm is to be expected instead of benefit. Counter irritation by means of mustard poultices applied to the angles of the jaws, and astringent and stimulating gargles are likewise of service. In some cases it is of use to paint the fauces with a solution of caustic (ʒss.—℥i.) every second day, or to touch the orifices of the eustachian tubes with solid caustic by means of Toynbee's caustic-holder; and it is often of service to remove the patient from a relaxing to a more bracing climate, from which the best results may be anticipated. I am not an advocate like Mr. Yearsley for the excision of the tonsils, unless they are so large as to interfere with the breathing, as I believe that most cases can be cured without any such operation. If the nostrils are stuffed, it is useful to make the patient close the mouth, and breathe entirely through the nose occasionally; and I have observed benefit from making him draw cold water, or a slightly astringent solution, through the nostrils twice or thrice daily. In adults the treatment is that of the affection which has produced the obstruction, combined with the use of local applications, such as those which I have just indicated.

In many cases the obstruction takes place at the upper or tympanic orifice of the tubes in connection with morbid conditions of the cavity of the tympanum.

I pass now to the last division of the subject, the mechanical treatment of obstructions—a method of treatment which is applicable to those arising from any morbid condition, except, perhaps, acute inflammation in the tympanic cavity. By this means alone we are enabled, in some instances to effect a cure—in those, namely, in which the obstruction is merely a mechanical one, or in which the inflammation which originally produced the obstructions has subsided, leaving the eustachian tubes blocked up. In these, the mechanical is the only treatment which is of any service. But in the majority of instances, the inflammation which was the exciting cause of the obstruction is is still in operation, in which case means must generally be adopted, such as those indicated in the previous sections, to remove it, as well as mechanical treatment to open the tube.

There are three methods of operating mechanically upon the eustachian tubes:—

1. By eustachian catheterism.
2. By Valsalva's experiment.
3. By Politzer's method.

1.—Eustachian catheterism has been so thoroughly described, and its merits so carefully discussed in standard works—especially in Kramer's work on diseases of the ear, which has been translated for the Sydenham Society, and in Mr. Yearsley's work, entitled "Deafness Practically Illustrated"—that I must for want of time, refer you to them; but I shall take the opportunity presently of contrasting its merits with those of the other methods.

2.—Valsalva's experiment, the nature of which I previously explained, has been used hitherto chiefly as a means of diagnosis, but I have been in the habit of extending its application to the treatment of obstruction of the tubes, and not unfrequently with success. I never prescribe this method till I have thoroughly satisfied myself, by personal observation, that the patient understands how to perform the operation. I then tell him to do so three or four times daily; cautioning him at the same time, not expire too forcibly for fear of injuring the delicate structures of the ear, and informing him that it is only by persevering in his efforts that he can hope to be successful in making air enter the tympanum, and in improving the hearing. It is only the other day that I was consulted by a lad who had obstruction of the eustachian tubes from thickening of the mucous membrane at their faucial orifices, and whom I treated according to this plan, to the exclusion of everything else, in order to test its efficacy. When he came to me, my watch was heard by him at the distance of two inches from the left ear, and three from the right. On his return a fortnight afterwards, the watch was heard six inches from the left ear, and more than two feet from the right. I could cite other cases did time permit, but I think I have said enough to show you that, in some instances at least, Valsalva's experiment is of value, not only as a means of diagnosis, but also in the way of treatment.

3.—The instrument of Dr. Politzer of Vienna, is that to which I wish more particularly to direct your attention. This instrument was described, and its use illustrated by cases, in a recent number of the Wiener Medizinische Woschenschrift; and it was from the perusal of a pamphlet containing a reprint of this paper, that I first had my attention directed to the subject.

The instrument consists of a piece of catheter, open at the end and several inches long, to which is attached a pear-shaped india-rubber bottle, about the size of the fist. The patient is told to take a mouthful of water, and the tube is then introduced about half an inch into one or other nostril. The nose is then closed by the finger and thumb of the left hand, while, with the right, the india-rubber bottle is suddenly and forcibly compressed, and the moment before it is squeezed, the patient is told to swallow the mouthful of water. Instead of a piece of catheter, I sometimes use an elastic tube, and not unfrequently I use an elastic tube alone—and for this purpose Toynbee's otoscope serves the purpose well, or a common elastic catheter, open at the end—and blow through the tube while the patient swallows, instead of propelling cold air through it by means of the india-rubber bottle; but I am not yet prepared to say which method is the most satisfactory.

The principle of this treatment is easily explained. The act of swallowing brings the levator and tensor palati muscles into play; the soft palate rises, thus separating the cavity of the nose from the mouth and throat, and the faucial orifices are opened. Now, as the patient swallows while the air is being driven through the nostrils, it, being unable to descend into the throat or mouth, and the faucial orifices being open, rushes up the eustachian tubes into the cavity of the tympanum, unless the obstruction resist the force of the current of air.

If the obstruction be removed, the patient experiences a fulness in the ears and his hearing is immediately improved, provided always that the obstruction was the cause of the deafness. The same treatment requires to be repeated every day, or every two or three days for some time, as the improvement in the hearing which takes place after each operation gradually diminishes, in the majority of cases, and it is only after repeated trials that it remains permanent. But if there be no obstruction of the tubes this operation not unfrequently diminishes the hearing power for the moment, owing to the drum being forced outwards too much, and to the congestion of the ear which results from the sudden rush of air into the cavity of the tympanum.

In some cases, and especially in those obstructions connected with thickening of the mucous membrane of the fauces, the sudden improvement is such as to astonish the patient, and to cause him to burst forth with an exclamation of gratitude for the service rendered; but you must be careful not to promise too much beforehand, unless you have had considerable experience of the class of cases which are likely to be improved by it.

In cases which are improved by this treatment you will naturally ask, How often is the operation to be repeated? My answer is, Repeat the operation till the hearing becomes perfect, and remains so for some time after the last inflation of air, or till no further improvement takes place for the last three or four operations, and no retrograde movement in the intervals.

Politzer's treatment possesses these advantages:—The operation is so easily performed that it can readily be done by any practical physician, and requires no special instruction. It causes neither pain nor uneasiness to the patient, so that it is not objected to, except by some young children and nervous females, and then only from fear. It is altogether devoid of danger, and there is no obstacle to its performance, unless the absurd timidity of some persons.

Eustachian catheterism, on the other hand, is a difficult operation to those who have not had much experience of it, and therefore necessitates special instruction. It generally causes more or less uneasiness, or even pain, if not very skilfully carried out. Some patients object to it very strongly, and for this reason it is occasionally inapplicable, and particularly in the case of young children, in whom mechanical treatment oftenest holds out a prospect of success. It is not altogether devoid of danger, and sometimes the bones of the nose exhibit abnormal forms, or the mucous membrane is the seat of tumours, which prevent the introduction of the catheter. But then, there are a few obstinate cases of obstruction which resist Politzer's treatment, and which yield ultimately to catheterism.

Valsalva's experiment has this advantage, that it can be carried on at home without the presence of the physician (although, to be sure, so in some cases can Politzer's treatment); it requires no apparatus, and it can thus be tried in conjunction with one of the other methods of treatment, especially if the patient reside in the country, and cannot come often to obtain advice.—*Glasgow Medical Journal, April* 1864, *p.* 1.

78.—INSUFFLATION OF POWDERED ALUM IN CHRONIC CATARRH OF THE MUCOUS MEMBRANE OF THE AUDITORY MEATUS AND TYMPANUM.

Mr. Hinton has found the application of alum in powder very very effective in certain forms of chronic catarrhal inflammation of the meatus and tympanum, which are otherwise often tedious in yielding to treatment. It is especially suitable when there exists a red and granular condition of the membrana tympani, or mucous membrane of the tympanum, with or without minute polypoid growths, such as often last from childhood even to the latter years of life, and which are so frequently met with, all acute symptoms having long subsided. The alum is easily introduced, after the ear has been gently syringed, either on a moistened camel's hair brush, or, which seems often more efficacious, it may be blown into the meatus through a piece of india-rubber tubing. After a few applications, efficiently made, the unhealthy condition will often be wholly removed, and with it the discharge, the hearing being at the same time, unless other lesions are present, greatly improved.—*Medical Times and Gazette, Jan.* 2, 1864, *p.* 11.

79.—A SIMPLE MEANS OF INFLATING THE TYMPANUM IN SOME FORMS OF DEAFNESS.

Mr. Hinton, Surgeon Aurist to Guy's Hospital, in forms of deafness dependent on obstruction of the Eustachian tubes from thickening of the mucous membrane, or from accumulation of mucus, has frequently used a method of inflating the tympanum, which has been recently introduced by Politzer, of Vienna. The plan of treatment is founded on Mr. Toynbee's discovery, that the Eustachian tube is naturally closed, but that it is opened by the tensor and levator palati muscles during the act of deglutition. Acting on this fact, Politzer conceived that air introduced with moderate pressure into the nostrils during the act of swallowing, the nose being at the same time closed, would be a very efficient means of overcoming any resistance which might exist in the internal auditory passages. During swallowing, the velum palati is raised, and, if the nostrils are also closed, the upper part of the pharynx forms a shut cavity, within which the in-pressed air operates with great effect, while the tube is at the same time expanded by the above-named muscles. The method Politzer proposed was the use of an india-rubber bottle, to which a portion of flexible bougie is attached in place of the ordinary nozzle. This being introduced into the nose, and the nostrils firmly closed over it, the patient, who has previously moistened his mouth with water, is told to swallow, and at the same moment the bottle is compressed by the hand of the surgeon. Air seldom fails to enter the tympanum, and its entrance may be heard by means of the ordinary otoscope, or its effect upon the membrana tympani witnessed through the speculum. Mr. Hinton has found that a simple piece of india-rubber tubing answers every purpose, and is often even more efficient. One end of the tube is introduced into the patient's nose, which is firmly closed over it, the other is placed in the surgeon's mouth, who then blows while the patient swallows. In the great majority of cases the method is at least as efficient as the introduction of the Eustachian catheter, while it is free from many of the objections to which indiscriminate Eustachian catheterism is open. The results are often very striking.—*Medical Times and Gazette, Jan. 2, 1864, p.* 10.

SYPHILITIC AFFECTIONS.

80.—SUBSTERNAL TENDERNESS A DIAGNOSTIC SIGN OF ACQUIRED SYPHILITIC TAINT.

By Dr. HENRY CRITCHLEY BRODRICK, Madras.

[Ricord casually alludes to substernal tenderness as indicative of the existence of constitutional syphilis. It was from this that Dr. Brodrick's attention was called to the symptom, and he has since carefully made observations on the subject.]

Substernal tenderness can only be detected by pressure over the bone, and when searching for it formerly, I used to *knead* the bone with the fore and middle fingers, carefully, from the manubrium to the xyphoid cartilage. In a case of suspected constitutional syphilis, if the patient be asked if he has got a pain in his breast bone, he will probably answer in the negative. The medical man should then *knead* the sternum carefully and gently along the whole of its course, and the tender spot will generally be found at the commencement of the lower third. With much practice and observation in this class of cases I now generally succeed in touching the tender spot at once, to the great surprise of the patient, previously quite ignorant of the existence of this tenderness. If substernal tenderness be found, I believe we are quite safe in assuming that the subject of it labours under acquired venereal taint, which may have been masked by divers symptoms, and be quite unsuspected both by the patient and the surgeon. It often furnishes a clue to the cause of very anomalous symptoms, and a most invaluable guide to us in treatment. For the past eighteen months I have been *kneading* patients' sternums most diligently, and have been not a little laughed at for the same by those not previoutly aware of the significance of substernal tenderness. The native doctors attached to the Malwa charitable dispensaries, which I superintend, all now practice this palpation in suspected cases to the very great benefit of their patients. Although the existence of substernal tenderness is, I believe, unerringly significant of venereal dyscrasia, it must be borne in mind that a patient may be constitutionally syphilitic without manifesting this particular sign. But, when detected, in it the physician has a very valuable guide for treatment. Substernal tenderness is, no doubt, produced by a periostal inflammation, slight in degree, and may be in its immediate effects, such as pain and tenderness, inappreciable to the patient. The iodide of potassium must be prescribed, under which the tenderness quickly subsides with other symptoms, which, mysterious before, this sign has taught us to decipher. I have hunted diseases to their source at once, in scores of cases, since I became aware of the existence and the importance of this diagnostic sign, and the rapid improvement of such cases under the specific treatment indicated above has invariably confirmed my diagnosis. I speak confidently on this point; that I am justified in this confidence any one can test easily in his practice. Constitutional syphilis prevails very largely in Malwa, so that I have a large field in which to practice palpation of diseased sterna amongst the sick coming to my dispensaries. I have had a limited experience of this diagnosing amongst Europeans, but I have found substernal tenderness in at least twenty such, and in as many the sign has led to the successful treatment of the disease it indicated. In a suspected case, then, look for this tenderness; it will usually be found at the commencement of the lower third, occasionally in the upper third, and very seldom in the space intermediate. When it is once made out, the subject of it should at once commence taking the iodide of potassium.—*Edinburgh Medical Journal, Nov.* 1863, *p.* 468.

MIDWIFERY,

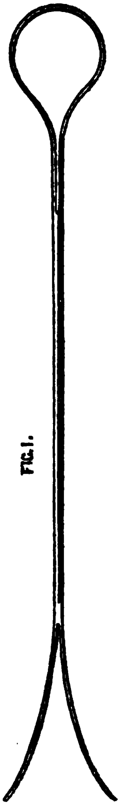
AND THE DISEASES OF WOMEN, ETC.

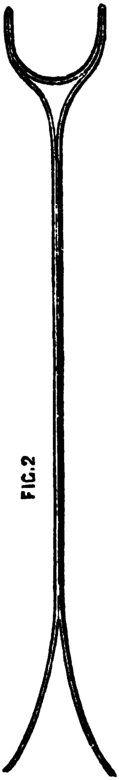
81.—ON THE USE OF WIRE LOOPS AS SUBSTITUTES FOR PESSARIES, &c.

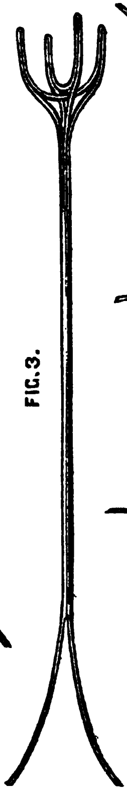
By Dr. Charles Clay, Manchester.

[Dr. Clay recommends these wire loops to suspend the womb and to rectify its malpositions when necessary, instead of the usual cantrivances, and we think that they are very likely to answer the purpose.]

Stem pessaries are certainly a great improvement, as they allow the vaginal canal a chance of being restored, and afford some hope to the patient that ultimately the instrument may be dispensed with, and a cure effected. By making the stem of elastic wire instead of porcelain was my suggestion in 1842; by this improvement I avoided the possibility of injury from fracture, which accident I have witnessed *more than once*, accompanied by very severe hemorrhage from a wound of the vagina, by the broken end of a porcelain stem pessary. The elasticity also of my instrument entirely does away with that vibratory jar often felt in walking, riding in conveyances, or in going down a flight of stairs, with a porcelain stem pessary. I have seldom found a case that has not yielded to this instrument when properly applied; but, like others, I have also found that the expense is too serious for a large proportion of those afflicted. Consequently I feel the greater pleasure in offering my new suggestion, which I will now endeavour to explain. There are three instruments, viz.: Fig. 1, a wire loop; Fig. 2, horseshoe wire; Fig. 3, a double horseshoe wire. These three are applicable to all the malpositions of the uterus. The wire is what is termed No. 13 or 14 in thickness, and when doubled up and soldered in the middle part about twelve to thirteen inches long. The mode of application is very simple, and will be easily understood. Suppose the case one of Antiversion, the instrument (Fig. 1) is introduced, guided by the index finger of the right hand, the loop is placed over the cervix, and then the os uteri drawn forward, assisted by the thumb pushing back the fundus uteri; when the uterus is







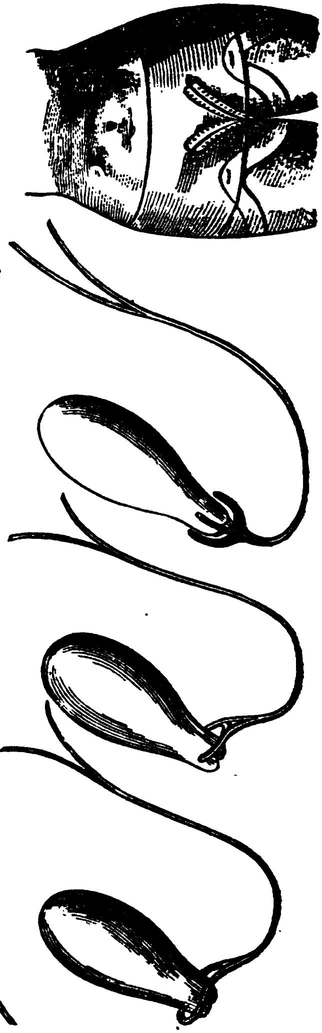


Fig. 1. For Antiversion. Fig. 2. For Retroversion. Fig. 3. For Prolapse.

placed in its natural position, the loop is closed partly by pressure with the finger and thumb, so as to hold it *without strangulating;* the stem of the instrument is then moulded by the finger to the curve of the vagina outwards, leaving the entrance of the urethra a little to one side; the moulding of the stem is now continued to the front of the abdomen (on which a bandage has been previously placed); the open ends of the wire are now fastened to the bandage by large pins, or the ends forced up two slots in the bandage, and when the points emerge at the top of the slot the points are bent downwards to secure them (as in Fig. 4). The narrow bandages round the thighs are to keep the main bandage from rising upwards. Suppose it a case of Retroversion, the horseshoe wire (Fig. 2) is used, introduced in the same way; the horseshoe is placed on the front of the cervix and pushed back to its proper position, which, when obtained, the stem is moulded and secured as before, as shown in the plate. If a case of Prolapse, the double horseshoe end is, when introduced, regulated so as to hold the os uteri, and then raised to its proper position, and the stem moulded as in the two former instances, and secured in the same way. It will be seen at once that all species of obliquity yield to these applications, and need no further description. To make the simplicity of these instruments and their cheapness more apparent, I present the Society with two sets of instruments, *one* more carefully finished and tinned over, the cost of the set of three being (without tradesmen's profits) about 9*d*., or 3*d*. each. The plain ones, which I make myself, and get soldered for about one penny each, answer every purpose required. Whether these instruments are tinned over or remain in their original condition as copper wire is of no moment, as in either case they keep much cleaner than in any other material. It will be seen at once that these instruments occupy so little space that there is not the slightest obstruction to means being used whilst wearing them for restoring the parts to their normal condition, or in keeping the vaginal canal cleanly at the same time the menstrual discharges are not impeded.—*Obstetrical Transactions, Vol. V.*, 1864, p. 181.

82.—REPORT OF FIVE THOUSAND CASES OF MIDWIFERY.

By J. Thompson, Esq., Kilmarnock.

[In 1855 Mr. Thompson published a Statistical Report of 3,300 cases of midwifery. Since that period he has attended 1,700 more, making a total of five thousand.]

From the commencement of practice I have attended six thousand cases, and used the forceps one hundred and thirty

times. In the whole of these cases the short forceps alone were employed, and with the exception of two cases, to which I shall afterwards have occasion to refer, were used not only with facility but with perfect success. If the long forceps are really required as often as is usually set forth in those works that treat of this subject, it seems at least a little strange, that not a single instance of the kind has occurred to me in the course of more than thirty years' experience, and in a practice as large as falls to the lot of most men. This will appear not less wonderful when I state, that I have met with almost every other case of conceivable difficulty, rare as some of these undoubtedly are; and yet I have never met with what is universally stated and believed to be a very common occurrence, namely, a case in which the use of the long forceps has been found to be indispensably necessary. I am perfectly ready to admit, that many things may have occurred in the practice of others that have never once happened to myself, and that it fairly comes within the domains of an ordinary possibility that the long forceps may have been required; but what I insist on is this, that if they are really necessary as often as some authors would have us believe, is there not something more than a possibility, is there not a probability, that out of six thousand cases, I must surely have met with a single and solitary instance, in which I could not have succeeded in effecting delivery without the assistance of the long forceps? But up to this hour no such case has presented itself; the short forceps have thoroughly and successfully answered every purpose. If I had been possessed of a pair of long forceps, it is very likely that some time or other I might have been induced to try them, and of course failed to perceive the fact which I now am endeavouring to establish; but never having felt the slightest necessity for them, I have not as yet thought proper to make such an investment. It is quite true that I have met with a case in which there was a difficulty in using the short forceps; but the same difficulty would have been equally great with the use of the long. This was a case in which there was an adipose tumour growing from the posterior wall of the vagina, about the size of an orange, and of course obstructing the passage. Here the head of the child was long detained above the brim of the pelvis, and where the use of any kind of forceps held out but little hope of immediate relief. Still in this apparently very unfavourable case, I succeeded in delivering with the short ones. On referring to my note-book I find the following statement regarding it:—At 8 p.m. of the 28th May, 1855, I was called to visit Mrs. S. In former confinements she had been remarkably easy, her pains being very natural and effective. On examination I discovered a tumour situated in the hollow of the sacrum, and as large as a full-sized orange. The head was felt presenting normally, but as high up as the finger could reach, the os uteri being as fully dilated as this condition of matters would allow. From the position of the tumour and the space which it occupied in the basin of the pelvis, it seemed at first impossible that delivery could be accomplished, unless the tumour could either be pushed upwards into the abdomen so as to allow of the descent of the head, or that the head itself should be broken up by the perforator and crotchet. I made repeated attempts to push the tumour out of the way, but failed altogether in changing its position in the slightest perceptible degree. In these circumstances I determined to wait a few hours, in order that I might see what progress would be made by the regular and unassisted efforts of nature. This I was induced the more readily to do, because the pains were neither frequent nor severe, and the case had not as yet by any means assumed a serious aspect. In the course of four hours the progress made was very little indeed. As the patient lived two miles out of town, and as I was unprovided with instruments of any kind, I resolved to return home, leaving word at the same time, that in the event of the pains becoming more urgent, I should be immediately sent for. At four a.m. of the 19th, her husband came with the information that his wife was now a great deal worse. Having supplied myself with forceps, perforator and crotchet, and trocar, I set out with considerable anxiety as to what the issue of this case was likely to be, more especially as the patient was of a very delicate constitution. I now found that the head had come somewhat lower, and that the tumour was becoming a little flattened by the pressure of the advancing head, which, however, was still too high to permit of an ear being felt. In the course of a short time I succeeded in the application of the forceps, the lock being fairly within the vagina when first applied. By acting gently and along with the pains, the head was delivered in about half an hour, the body soon followed, and shortly afterwards the placenta. The mother seemed a little exhausted, and the pulse was as high as 120; the child was healthy and well. The tumour was now felt greatly flattened, and did not appear to occupy one half of the space which it did at the beginning of the labour. Mrs. S., made an excellent recovery, without the occurrence of a single unpleasant symptom, and in little more than the usual time. On the 9th June I made another examination, and found the tumour as large as before, the os uteri being closely pressed up against the pubes. I have since delivered this patient a second time in precisely similar circumstances, and by the same means, without the occurrence of any untoward symptom.

Almost every one who has written on this subject agrees in representing the long forceps as a much more formidable instrument than the short ones.

Smellie, the inventor of the long forceps, was so fully aware of the danger of fatal contusion and laceration of the uterus and vagina necessarily incurred by the application of the long forceps, when the head of the child remained above the brim of the pelvis, that he did not venture to exhibit the instrument to his pupils, from the dread that it would be misapplied; and it appears he always used and recommended the short forceps.

Denman says—"It behoveth every person who may use instruments in the practice of midwifery, to be well convinced of the necessity before they are used, and to be extremely careful in their use that he does not create new evils, or aggravate those which might be existing. The preference which ought to be given, in reason, of one kind of forceps to another, is merely because one instrument may be more handy and convenient than another; for an intelligent and skilful man would be able to apply and use those of any form or size in such a manner that they should answer his purpose; as an expert surgeon would be able to amputate a limb with a knife of any kind."

Dr. Blundell states, that in violent hands "the long forceps is a tremendous instrument; force kills the child, force bruises the soft parts, force occasions mortification, force breaks open the neck of the bladder, force crushes the nerves: beware of force therefore—*arte, non vi.*"

Dr. Lee says that "when Dr. Hamilton was at one time on a visit to London, the use of the long forceps was one of the subjects of conversation I had with him, and I was astonished when he informed me, that he had entirely for some time laid aside the short forceps. On further inquiry, however, it appeared that in no case did he ever use the long forceps until an ear could be felt; thus admitting that he never employed the long forceps where it was possible to deliver with the short ones, and to derive all the advantage which could be obtained from such means with much less risk."

If this observation of Dr. Hamilton's be correct, and it is in perfect coincidence with my own experience and observation, then perhaps I may be permitted to ask where is the necessity for the long forceps at all; certainly in no case of this kind (and it is the very kind of case selected by this gentleman, in which he thinks the long forceps ought to be used) would any competent authority at the present day ever think of delivering by any means but the short forceps. It is no doubt quite true that it would be perfectly easy to deliver by the long forceps; but if it can be done quite as well with the short ones, then surely it will be admitted that there is no necessity whatever for the long forceps. In regard to those cases which are set forth in every work on the subject which I have happened to peruse, and where the presentation of the head through some cause or other remains so long above the brim of the pelvis, that from danger to the mother it becomes a matter of imperious necessity no longer to delay, but to deliver by the long forceps, I think I may, under a becoming sense of the importance and responsibility of the statement, venture to affirm that they are altogether fictitious and illusory. This state of matters could only arise from one of two causes—either the brim of the pelvis was so contracted as to render it physically impossible for the head to pass through ; or the action of the uterus itself was so irregular and spasmodic as for many hours to make little perceptible progress. In the first case, I think, no one will attempt to deny that it would be a very difficult thing indeed to apply the forceps ; but even granting that this difficulty had been overcome, would it not be a very dangerous thing to use them ? but setting aside the danger altogether, would not such an attempt to bring down the head prove entirely fruitless and abortive ? In the second case, where the head is detained above the brim through the instrumentality of irregular action, not only does such a kind of labour entail no danger whatever to the mother, but in consequence of the os uteri being but partially dilated the application of the forceps becomes all but impossible, more especially with a presentation so high up in the pelvis. In the first instance the application of the forceps could answer no purpose of a useful kind ; in the second, the exercise of patience will eventually, effectually, and safely cure the difficulty, and at least bring the head within easy and successful reach of the short forceps. In the whole of the six thousand cases which constitute the amount of my private practice, including at the same time every case of difficulty in which I was called upon to assist others, I have only met with seven instances in which I did not readily succeed with the short forceps. The first was a case where the arm had come down along with the head, and where the practitioner in attendance was satisfied that the fœtus was dead before I arrived. Here the forceps had been tried and failed, and delivery was effected by the perforator and crotchet. The second was delivered by the same means, the forceps having failed in former confinements. The third case was one of greater difficulty than usual. This was the patient's second confinement. Her first confinement had been long and tedious ; sloughing of the vagina had taken place ; and as a necessary consequence, adhesions had occurred to such an extent as nearly to close the vagina. I had seen this patient about two months after her first confinement, and had then used the bistoury pretty freely, but as afterwards appeared, not to a sufficient extent. Her next confinement took place at a considerable distance from this, under the care of an excellent and experienced practitioner. When I arrived at the patient's house, I found that she had been ill for many hours, and that her distress was very urgent indeed. On examination I ascertained that the vagina was still so much contracted, as to render it impossible for the head to pass. Again I used the bistoury in different places, and to such an extent as to leave no doubt of a sufficient permeability. Immediately afterwards I applied a pair of long forceps which the medical attendant had with him, but failed to bring the head into the basin of the pelvis. My friend now made a patient and protracted attempt, but was not more successful. The perforator and crotchet were applied, and with these I succeeded in delivering the patient, who recovered without a single bad symptom.

In the fourth case I was asked by a friend to visit a patient whom he had just left. He told me he had used the long forceps, but had not succeeded in delivery. I took with me the short forceps, perforator and crotchet. The short forceps I applied with great readiness; but with all the strength I could command, I could not bring the head nearer. We sent for another medical friend, and with him a stronger pair of long forceps, as my friend who had charge of the case was very unwilling to deliver by any other means. Our mutual friend tried his stronger pair of long forceps, but with no better success, so that we were shut up to the use of the crotchet. The patient, though greatly exhausted, made a good recovery.

The next three cases occurred in my own practice. In the present case, the patient told me that in all her former labours she had been delivered with the forceps. She had been ill for a good many hours before I was sent for. On examination I found the head presenting, pretty high up, but with the os uteri fully dilated. After waiting for a considerable time and finding that no progress was made, I applied the short forceps, but soon discovered that any force I could employ was of no apparent advantage. In these circumstances I sent for a friend to assist me, and share in the responsibility of the management of the case. He applied the long forceps, but could not bring the head any nearer. I then applied the perforator, when a gush of water took place, showing that the head was hydrocephalic. Delivery was speedily effected with the crotchet, and the mother made a rapid recovery.

The next two cases I shall transcribe from my note-book as they were written down at the time. On the evening of the 29th October I was sent for to visit Mrs. Y. I found that the membranes had been ruptured, and that the liquor amnii had escaped. The pains were few and trifling in their nature. I remained with her all night; but no appearance of effective pains setting in, I left with instructions to send whenever she should become worse. On the evening of the 31st I was again sent for, and arrived at her house at half past six o'clock. I found on examination the head presenting somewhat high up, with the os uteri fully dilated. The pains were grinding and severe, the patient restless and uneasy, often desirous of getting up; and it soon became evident that the labour would be tedious, inasmuch as little progress was being made. For four hours this state of matters continued, without any prospect of a termination to her sufferings. A messenger was now despatched for the forceps; but having four miles to ride each way, an hour and a quarter elapsed before he arrived. For some time previously the respiration had been getting quicker, and the pulse had risen to 120. Before the arrival of the forceps she was in such a state of distress from heat and the continuation of the same grinding pains, that she persisted in getting out of bed and divesting herself of almost every article of clothing. Having made arrangements for proceeding with the application of the forceps, I endeavoured to ascertain the exact position of the ear; but in this I entirely failed. I then attempted to pass the first blade in the ordinary direction, but from the jamming of the head in the bones of the pelvis I found this to be impossible, without using a greater degree of force than I was willing to employ. I changed the direction of the point of the blade towards the sacro-iliac junction, and in this position it passed onwards, but apparently farther inwards than is usual. The next blade was passed with considerable difficulty in the opposite direction; but before the handles could be locked, the first blade had to be withdrawn for a little. On making gentle traction they almost immediately slipped. The blades were reapplied in a different direction, but with no better success than at first. These trials were continued to be repeated for about three quarters of an hour, the instruments slipping almost as soon as they began to be used. It had now become painfully evident that the patient could not be delivered with the forceps. I therefore determined to send for the perforator and crotchet and another practitioner to assist. The patient by this time had become somewhat exhausted, and her pulse quick and feeble. Soon afterwards I thought I observed her belching up a mouthful of something like coffee-ground matter. The patient continued to get rapidly worse, and was evidently now in a sinking state. Heat was applied to the extremities but cordials of every kind she stedfastly refused. From this period she continued to get weaker and weaker, and died about 2 a.m. of the 1st November, that is, about two and a half hours after I had laid the forceps aside, and a few minutes before the arrival of the other practitioner. No post-mortem examination was permitted.

The next case in many respects was exceedingly like the one just detailed, though fortunately attended with a happier termination. Mrs. L. had been complaining for some time before I saw her. When first visited, the os uteri was fully dilated; but the head had become impacted, and no progress was afterwards made for several hours. The forceps were proposed and immediately procured. Not being able to reach the ear, I pushed the first blade onwards in the usual direction; but in consequence of the jamming of the head, it immediately slipped either upwards or downwards, while the other blade was introduced in a corresponding direction, so as to permit of their being locked. On traction being made, they lost their hold and had to be withdrawn. Trial after trial was made for upwards of an hour, uniformly with the same result, while very little progress was made in disengaging the head from its position. During the whole of this time the pulse remained steadily at 84. Tired and disappointed, I went and procured the assistance of a medical friend. With long and short forceps he made repeated attempts to bring down the head, but with the same unfortunate result; for none of the instruments would retain their grasp, but slipped as soon as they had begun to be used. In these circumstances we agreed to leave the patient for two or three hours to the unassisted efforts of nature. At the termination of this period I returned, and found that the pains were strong and bearing down, but that little or no progress had been made. I tried the short forceps again, and again they slipped; at length by repeated efforts I succeeded in bringing down the head. The child, one of the largest I had ever seen, at first showed little signs of life, but was at last restored to vigour and activity. The ear towards which the first blade was usually attempted to be passed, was considerably injured, but in other respects it was comparatively little. The mother passed a good night, and made a good recovery. Twice before she had been delivered with forceps, and on this occasion she was eighteen hours in severe labour. This case very much resembled the last; they were unique in my experience. In both cases the pelves were probably under the average dimensions, and when the children were unusually large the difficulties occurred. The fatal termination in the case of Mrs. Y. was occasioned in all likelihood by a more delicate constitution.

I have thought often and anxiously about the nature of the cause which prevented the forceps in these two cases from taking a secure grasp of the head, and the only conclusion at which I can arrive, is, that the blades of the forceps were too short fairly to embrace heads of more than the usual size. On this account I have been led to measure the length of the blade in a considerable number of instruments in the hands of the maker, and I find a very general agreement as to size, that is, that the length of the blade measures from 5¼ to 5½ inches. Now it appears to me that in the two cases just narrated, I failed in consequence of the shortness of the blade of the forceps employed; and that if I had had a pair measuring at least 6 inches, a secure grasp of the head might have been obtained, and the labour terminated in a manner much more agreeable and satisfactory to all parties. It does not appear to be quite consistent with the ordinary demands of science, that one size of blade should be expected to suit all sizes of heads. In no department of mechanics would such a law be found to hold good. If I might venture on giving an opinion on this interesting and important subject, it would be this—that in order to be provided for all contingencies, every practitioner should be furnished with two or three pairs of forceps with blades varying from 5 to 6 inches in length, so that when any difficulty occurred he might be able to select those most likely to be required by the exigencies of the case.

A perusal of the history of these cases will satisfy any one, that at least my experience of the use of the long forceps has been anything but satisfactory. In not a single instance in which I have seen them applied, did they succeed; and in every one of the cases where they were applied and failed, I could quite as readily and as effectively have used the short forceps: of course I only speak of what I have personally seen and experienced.

But briefly to recapitulate; I have used the short forceps one hundred and thirty times with comparative ease and facility, and with almost universal success. In four cases I failed with the short forceps, but in none of these was the success any greater with the long ones. In every case where I have seen the long forceps employed, I was able quite as readily and effectually to apply the short ones. If such then is the brief and eventful history of the long forceps in the long catalogue of six thousand cases, I am forced to the conclusion, that in so far at least as my personal experience is concerned, they have been entirely superfluous and unnecessary, and may without injury to the cause of practical obstetricy be obliterated from the list of obstetrical instruments.—*Glasgow Medical Journal, April* 1864, *p.* 27.

83.—TWO CASES OF INVERSIO UTERI, WITH REMARKS.

By Dr. A. L. Kelly.

(Read before the Glasgow Southern Medical Society.)

[We consider the two following cases interesting. Care should render such cases very uncommon, but many trivial circumstances may unite in causing them.]

On the 21st April, 1852, I was requested to visit Mrs. M'F., aged nineteen, who was reported to be in labour of her first child. I found her tall, well formed, and apparently healthy—though much fatigued, owing to considerable suffering during the previous night. On examination per vaginam the os uteri was found well dilated, its edges thin, the vagina rather hot, presentation natural; the uterine contractions also recurred very regularly, but not with such power nor of such duration as might have been expected. Hence the labour, though steadily progressive, was slow and lingering. At length, on the morning of the 22nd, it became so feeble and ineffectual as to render instrumental aid necessary for the patient's safety. After emptying the bladder, warming and oiling the forceps, I easily and quickly applied them, as the head was low, almost upon the perineum; there being, however, no pains, steady and slow tractive efforts were employed to extract the head, which was no sooner accomplished, than a large gush of blood was poured upon the bed. I then quickly asked the attendant at the bedside to lay her hand upon the belly, and without a moment's hesitation she somewhat rashly and roughly pressed both hands *into*, rather than on the belly (it may be observed that in her right hand she held a towel, so that her fist was closed). This firm pressure seemed to arrest the flooding, and to have the effect of producing pain, as the body of the child (living) was expelled with very slight manual help; but again hemorrhage ensued, with sighing, restlessness, pulse extremely weak, and that exsanguine aspect, as well as general coldness of the whole surface of the body, which indicates imminent peril. Having given her a quantity of whisky, and made her head level with her body, I passed my finger towards the vulva, which immediately touched what I supposed and even considered the placenta; but after tying the cord, and separating the child from the mother, I laid my hand upon the belly, but finding no hard or rounded uterus, the thought of what might be at once flashed across my mind. Accordingly, to my extreme regret, I found lying upon the bed this soft mass formerly felt; and beneath, the firm, hard, oval, unmistakable, entire inverted uterus protruding quite externally. At this point, observing the dangerous condition of the patient both from the shock of such an accident and from the exhausting loss of blood, I requested the assistance of my friend Dr. Tindal, who I was aware had had a similar case, and his attendance, I am happy to say, was prompt. Our first thought was that it would be best, and we even endeavoured, before returning the displaced organ, to remove the placenta. To distinguish, however, placenta and walls of uterus, either by appearance or feel, and to separate the former with impunity to the latter, so firmly adherent whilst covered with blood, was a more difficult and hazardous task than one may feel disposed to credit, and than we were willing to risk. With right hand doubled up, therefore, I pressed steadily and firmly against the fundus, or most dependent part of the whole mass, which very soon began to yield, and glided upwards, followed by the hand, until complete reduction and normal restoration was effected. The placental mass was now speedily and easily peeled off, in doing which the internal irritation re-excited the contractile powers of the uterus, and the retained placenta was expelled, with which the hand was withdrawn. Thereupon more whisky was given, which soon rallied her from the shock and loss of blood. She was further directed to have small and frequent doses of it, with forty drops of laudanum immediately—to be repeated in two or three hours after if pain was complained of The abdomen was firmly bandaged, strict quietude enjoined, and careful avoidance of everything likely to produce a recurrence of the accident was insisted upon. Three hours afterwards when I called she was calm and composed, said she did not feel much pain; and here it may be remarked that to my surprise she never throughout complained of much pain, except at the moment when the attendant's hands were placed over the belly, and which I suppose to have arisen from the displacement of the organ. Subsequent visits I need not particularize, as no untoward circumstance occurred; and in a few days she was out of danger, and her health well and much sooner established than the fondest expectations could have warranted.

The second case occurred on the 9th February, 1863, when I was hastily called to see Mrs. G., a healthy, stout, well-proportioned woman. She was, I found, attended by a self-taught midwife, who stated that the presentation and birth of the child were natural, about two hours previous to my arrival, but that the after-birth was slow in making its appearance, although she had rubbed the belly, produced coughing and sneezing by a pretty free administration of snuff, and who also reluctantly confessed to having pulled the cord, as she had felt it yield; and added, while turning off the blankets, "There it is, but it'll neither come off, nor go where it came from." In this case hemorrhage, although considerable, had not been so great, nor the shock so severe or prostrating, as in the former case. The patient spoke quietly but firmly whenever I entered the room (I learned afterwards, wine and whisky had been poured into her *ad libitum* whenever she seemed faintish). At the same time her pulse was small and feeble, face pale, chilly, and damp; besides she complained much of pain in her belly, and felt "just going to die"—such were her own words. I hastily pushed the nurse aside, and grasped the mass with the cord still attached, which felt much smaller and softer than in the previous case. Again careful and steady pressure in the same manner and direction as afore-mentioned, gradually but slowly effected restitution; with a sudden and distinct bound the fundus resumed its normal condition, when with ordinary care the placenta was now entirely removed, and expelled with the hand. Slight hemorrhage ensued, but was arrested by friction and pressure over the uterus, and subsequently by the application of a firm bandage. She then complained of nausea and pain, which was quieted by frequent doses of dessert-spoonfuls of whisky and soda water, followed soon after by the administration of fifteen drops of laudanum every hour until she has taken four doses, when she became drowsy and slept two hours. On awaking she pronounced herself better. The following night, however, vomiting and severe pain in the abdomen set in, with hot skin, quick feeble pulse, dry tongue, and slight cerebral excitement. On examining the breasts, milk was found present, and during the abstraction of a small quantity, uterine irritation was excited, which expelled several clots of blood; her urine was drawn off by the catheter, cold applied to the head, a cataplasm of mustard and linseed meal over the whole abdomen, and a teaspoonful of a mixture containing magnesia, laudanum, hydrocyanic acid, syrup, and water, was given every hour for some time. Relief and improvement were the results, and subsequently her progress was tardy, yet very satisfactory.

Such then is a brief and incomplete sketch, very slightly extended by the aid of memory, from jottings taken during my attendance and for my own use. It is however, as far as it goes, a faithful record of the only cases I have ever seen, or have any desire to see, of a like kind; and I have sought to place it on record, because, though rare, such instances do arise, are accompanied by alarming symptoms, yet are not invariably followed by the pleasing results which I have related. Both were those of complete inversion, which occurring after delivery may arise from many causes; but it is said to be almost always traceable to violent removal of the placenta. In the former of the two above narrated, I consider that there were predisposing causes in the large pelvis, the adhesion of the placenta to the fundus uteri, and the flaccid condition of the uterine walls, whilst the immediate and exciting cause was the quick thrust, or rather plunge, of the attendant's half-closed fist into the patient's belly. Probably this only developed or produced mere depression of the fundus, yet sufficiently so as to induce uterine contraction of one part, whilst there was entire atony of another. Moreover, by continuing the pressure the force of the pain was so increased, that, in addition to nearly expelling the child, it carried downwards externally the fundus and body of the uterus, with its whole contents. Indeed, the comparative ease with which the whole mass was expelled confirms me in the opinion that the fundus was depressed before, and ultimately quite inverted as the child was expelled. In the latter case the only discoverable cause, as in fact the confession made somewhat tardily indicates without a shadow of room for doubt, was violent traction of the cord. So much then as to the causes. We must call attention to the mode of operation, and it will have been observed that in both cases restitution was effected with the placenta in the former adherent, in the second partially so. Now, while the most expert and easy method of performing any operation well merits grave consideration, it will be granted that the safety and the future good health of the patient are the primary and essential points. That being the case, the question comes to be here—Is it not necessary, or advisable, to replace the organ immediately, since the longer the delay, the more serious and less practicable does the operation become? But perhaps it may be asked, why we should not, in order to hasten or facilitate reduction, at once remove the placenta? And to this my reply is—First, that had it been partially detached, it might have been expedient or easier to do so, but that, being adherent, the treatment above adopted was right, because the internal surface of the uterus would have been exposed to atmospheric influence, predisposing its taking on inflammation with its consequences. Second, hemorrhage must have been greater, as the mouths of the vessels would have been laid open; or if contraction had taken place so as to arrest the bleeding, then the difficulty in accomplishing restoration would have been much greater. Third, the force necessary for reduction exerted against the bare uterine parietes, must have bruised, and might even have ruptured them. And lastly, the time occupied in peeling off the placenta, would have been time gained for greater and firmer contraction. For these reasons, accordingly, as well as because of the success which attended the procedure here advocated, I propose, if I ever again have the misfortune to meet a similar case, to follow the same plan.

Let me add further, that in both cases great depression sinking "as if going to die," was the most prominent and sudden symptom, dependent I have no doubt, upon the severe shock communicated to the nervous system, and which was overcome by small quantities of whisky very frequently administered. Small quantities of stimulants, therefore, as well as small doses of medicine, I would strenuously advise, since they are well borne by the stomach, and less likely to induce nausea. The laudanum given, in like manner, is perhaps advisable to mitigate the effects of the shock, relieve pain, and even prevent the probability of any recurrence of the same accident; for this must be well watched a considerable time, seeing that there is said to be a marked tendency to inversion for some time afterwards. I do, however, incline to the belief, that if restitution is quite complete before the hand is expelled or withdrawn from the uterus, and the usual subsequent precautions taken, such a circumstance would not very readily occur. —*Glasgow Medical Journal, Jan.* 1864, *p.* 426.

84.--CHRONIC INVERSION OF THE UTERUS.

Case under the care of — SHEPPARD, ESQ., Surgeon to the Worcester Hospital.

[The patient was aged twenty-five, and was delivered of an illegitimate child in May, 1859.]

The labour was lingering, and the removal of the placenta by the midwife was followed by severe hemorrhage. On the fifth day, while in the act of passing water, she complained of "something coming down," and her mother states that the womb made its appearance externally, and was pushed back by the midwife. The pain was so severe, and the prostration so alarming, that a medical man was sent for, who attended after some hours had elapsed, but no examination was made. From this date she became a patient at the infirmary (in December, 1862), she suffered almost constantly from menorrhagia, and lost great quantities of blood. When I first saw her she was perfectly blanched, and in the weakest possible condition. Feeling satisfied that there was some specific cause for this hemorrhage, I at once made the usual examination, and found a small body about the size of the virgin uterus occupying the vagina. The finger could be passed completely round the broad neck of the tumour, which was closely encircled by the os uteri. Seen through the speculum, it had a soft vascular appearance, and when touched it was extremely sensitive and inclined to bleed. The uterine sound could not be made to pass more than a quarter of an inch within the cervix. The finger in the rectum detected the absence of the body of the uterus from its natural position in the pelvis, but could recognise a small movable tumour in the vagina. The patient was confined to her bed up to January 13 of the present year; but notwithstanding the use of astringents internally and locally, hemorrhage still continued at brief intervals, and rendered operative interference necessary. Perhaps her countenance was a little less blanched than when I first saw her, but there was no difference in the size, vascularity, or sensitiveness of the tumour.

My colleagues having carefully examined the case with me, we came to the conclusion that the tumour was an inverted uterus, and that as no prospect existed of the patient recovering her health while it remained in the present state, its reduction or removal was imperative. The small size and firmly-contracted state of the organ, as also the length of time it had been inverted, rendered its reduction an impossibility. I therefore proceeded, under chloroform, to encircle it by means of the écraseur with an annealed wire; this was easily accomplished, but the wire giving way, I brought down the tumour with a pair of forceps, applied a whip-cord ligature, and separated it with curved scissors. The patient had no bad symptoms after the operation, but, owing to her weak condition from continual loss of blood, convalescence was very slow. The ligature came away at the end of a fortnight, but my patient remained in the Infirmary for another month.

When I last saw her, about a fortnight ago, she appeared in tolerable health, and capable of doing light work.

The uterus is preserved in the museum of the Hospital; it is about the size of a small walnut. A part of the lateral ligaments and Fallopian tubes are well shown.—*Medical Times and Gazette, Dec.* 19, 1863, *p.* 643.

85.—ON RECTIFICATION OF OCCIPITO-POSTERIOR POSITIONS OF THE FŒTAL HEAD.

By Dr. William Leishman, Physician to the Glasgow Royal Infirmary and to the University Lying-in-Hospital.

[Can an occipito-posterior position be rectified by artificial means? If so, at what period, and in what manner, can the rectification be effected? In what circumstances are we justified in interfering with the course of nature in order to effect the change? There is no doubt whatever that under certain circumstances the position of the head may be rectified, as many of the most eminent accoucheurs can testify.]

As regards the period of labour at which the rectification may be effected, we find much difference of opinion among accoucheurs. By many writers the question is put in such a way that we are entitled to infer from their statement that the change may be effected with equal ease, or rather with equal difficulty, at any stage of labour. If this is the view which is intended to be conveyed by those who so express themselves, it is certainly erroneous; and of this we may easily convince ourselves by attempting the rectification of the ordinary third position (the *bregmato-cotyloid* of Dr. West) in a few cases, when we shall find that the head cannot in ordinary circumstances be turned back until it has reached that stage of the labour when nature of herself induces the rotation. In such cases, I have never been conscious of aiding the rotation one whit, although I have actually felt the head turn while I was manipulating. A professional friend lately told me of a case of occipito-posterior position in which he repeatedly attempted, by means of the forceps, to bring about the turn, but without avail. Having paused for a few minutes, but without withdrawing the blades of the instrument, his attention was attracted by a rattling noise made by the forceps, the blades at the same time slipping away from their hold of the head, and, on examination, he found that nature had done with ease what he had been in vain attempting to do, and that the head had completed its movement of rotation during a single pain. Such facts as these show that rotation cannot be effected in all stages of labour, but that it is only under certain circumstances that we can interfere with a reasonable prospect of success. My impression is, that rotation can only be effected by artificial means when the head is free above the brim, or when it has quite descended to the floor of the pelvis. In the former case we should be most likely to succeed with long, straight forceps, the handle being pressed back against the perineum, and gently turned until the head was brought into the desired position. The practical advantage of operation in such a case would be to place the head in the most suitable direction, if nature did not do so of her own accord—as in the case, to take an extreme example, of an obliquely-distended pelvis, in which the long diameter of the head was in the least favourable position. It must be rarely, however, that circumstances call for operative interference at this stage of labour; and it must be remembered that we are now talking merely of the circumstances in which the operation is feasible, and not of cases in which it is advisable. Whatever the circumstances may be in which we attempt to rectify the position of the head, there can be no doubt of the fact that a most important influence is exercised by the state of the uterus as regards the waters. If these have completely escaped, and the uterus tightly surrounds the body of the child, the rotation of the body is interfered with, and consequently that of the head is rendered much more difficult.

The circumstances, probably, in which we are warranted in interfering, with a better prospect of success, are to be found in that stage of labour where the head, in the third or fourth position, has descended to the floor of the pelvis. Under ordinary circumstances, rotation will spontaneously occur; but in those cases in which the occiput is simply arrested, and the forehead comes lower and lower until we can distinctly feel the orbits and nose, we have every reason to fear that rotation will not take place. I gather from Dr. West's paper that he considers the *fronto-cotyloid* as an original position—a position, that is to say, in which the frontal end of the long diameter precedes the occipital during the whole course of labour. He admits, however, that the ordinary *bregmato-cotyloid* variety may become changed during labour into the *fronto-cotyloid* in the manner described by most modern accoucheurs. If this be so, the position may be vicious from the beginning, or it may become so by a perversion of the expulsive forces during the course of labour. If we admit the occurrence of the former class of cases, I think they must be held as of rare occurrence while the latter form the general rule.

It will be remembered that, during the descent of the head in this position, the expulsive force is directed against the occiput, the spinal column being articulated much nearer it than the frontal end of the head. So long, therefore, as the former meets with no resistance, it passes downwards in the axis of the brim; but, when it experiences the opposing force at or near the floor of the pelvis, one of two things may happen: it may either move in the direction of the resultant of the combined forces—*i.e.*, from above downwards, and from before forwards; or it may, from some peculiarity in the form or relations of the parts, simply become arrested by the first impediment it meets with, while the force becomes misdirected against the forehead in the following stage of labour. Perhaps the simplest view which we can take of this is to consider the long diameter of the head as a lever. In the ordinary progress of such a case as we are considering, the fulcrum is at the frontal end, and the power near the occipital end of this diameter. So long as the resistance which is opposed to the descent of the frontal end, and which constitutes the fulcrum, is sufficient for this purpose, the descent of the occipital end continues, and the rotation is merely a more complicated phase of this mechanism. But if the occiput be arrested in its descent, the fulcrum is transferred to the occipital, and the power to the frontal end, respectively, the result being the gradual descent of the forehead, and the more than probable termination of the labour in this position.

Various methods have been suggested by means of which the artificial rectification of the head may be effected, these methods being either manual or instrumental, or both of these combined. It is perfectly clear that, if the forehead has come down, no mere rotation can bring about the change which we desire to effect. Rotation must in all cases be so managed that it is combined with a descent of the occiput, and a corresponding retreat of the forehead. Indeed, as regards artificial rectification, it would probably be more correct to say that the latter was the movement of primary importance, while the rotation was really a matter of secondary consideration. That most writers have taken this view of the case, is evident from the fact that we are almost always directed to manipulate so as to cause the forehead to pass upwards.

[Instrumental aid not unfrequently becomes necessary in these cases, yet the forceps alone are as a rule unsuitable, as they can only act by dragging the head downwards in the position it then occupies.]

The method which is most correct in theory, and also—to judge by the cases cited by Dr. West—most successful in practice, is that recommended and practised by him. The operation consists in a combination of manual and instrumental forces, by the action of which the forehead moves upwards and backwards, and rotation occurs. The instrument employed is the vectis, which, with the aid of the finger, can fulfil all the conditions necessary for rotation in a much more satisfactory manner than the fingers alone or the forceps can do. This mode of procedure is, however, only to be adopted when we fail with the hand. The importance of this subject demands a more detailed examination of Dr. West's views.

On the subject of artificial rectification, I shall let Dr. West speak for himself, as in the detail of these cases his theory and method of operating will be much more clearly shown, while their importance in a practical point of view will be more distinctly recognized. The cases as detailed are notes made at the time in his midwifery register.—

"*Case* 1.—*Face to Pubes.—Third Position of Cranium.—Artificial Rectification.*—Dec. 28, 1853.—Could feel nothing but anterior fontanelle, which was nearly closed, and turned towards left side, and the sutures were scarcely perceptible. The anterior fontanelle feeling like the posterior came down, and presently I felt the nose at the symphysis. Thinking the pains did not press the head down while in this position, I turned the face to the left side again; and then, when I could feel an ear at the pubes, I pressed behind it till the occiput came to the arch. Labour over with two more pains. *Observe*—In rectifying the position of the head, after the nose has been felt at the symphysis, it is not sufficient to merely bring the ear to the symphysis, as the frontal end of the head will still be lowest, and the head will merely be brought back to the vicious position which had resulted in the turn of the face to the pubes, and if so left, will again get wrong, or perhaps get impacted, the head being with its whole length in the transverse diameter of the outlet. It is necessary to bring the ear to the side; and at the same time it is expedient to raise up the anterior fontanelle, that the *vertex* may descend; for it must be borne in mind that *that* is the material point to be effected.

"*Case* 2.—*Face to Pubes—Fourth Position of Cranium—Artificial Rectification.* - January 27, 1854.—On first examining, I found the head in the fourth position, a considerable portion of the frontal bones down, the anterior fontanelle towards the right side, and the posterior fontanelle out of reach altogether, the right coronal suture pointing to symphysis pubis. After the next pain, I found the *frontal* suture at the symphysis, the anterior fontanelle being central, or nearly so, and the right coronal suture far away to the left side of the symphysis. Turned the anterior fontanelle backwards, and waited for another pain. The forehead came forwards again. Turned it backwards again, at the same time raising it up until I felt the posterior fontanelle, the right *lambdoidal* suture pointing to the symphysis. With two more pains the labour was over, with the occiput under the arch.

"*Case* 3.—*Right Ear at Pubes—Artificial Rectification.*—February 12, 1855.—Felt anterior fontanelle near coccyx, and right ear at pubes. Felt part of the cheek on right side of symphysis. Head had escaped from os uteri. Pains had been very strong for some time, with the liquor amnii evacuated. Thinking that because the forehead had got down into the pelvis the occiput would not be able to find its way to the arch, and that the forehead would turn there instead, I pressed the ear to the right side, and the occiput coming down, passed under the arch. The labour was completed immediately after. As the revolution of the head was in the interior fontanelle, that part was felt in exactly the same position, with reference to the coccyx, after the rectification as before.

"*Case* 4.—February 13, 1853.—Feeling anterior fontanelle low down, and slightly to the left of the coccyx, I felt round the right side for the posterior fontanelle, but could not find it. I then passed the finger to the symphysis, thinking to find the ear, when it struck on the left eye (close to the symphysis, but slightly to the right of it), and on the root of the nose (close to the left side of it). The os uteri was fully dilated, the head on perineum, and the woman had been only two or three hours in labour. I therefore did not attempt rectification, and in three or four pains the child was born, the frontal suture having passed rather nearer the central line of the pelvis."

The first three of the above are cases in which rectification was effected without much difficulty, by manual interference alone. The details are eminently interesting and instructive. Whether the cases are managed according to the method of West and most others, by pushing the forehead up, or in accordance with the direction given by Meigs, by pulling the occiput down, the result is the same. The rotation, as I have said, is a secondary matter, as the great point which we must strive to attain is to put the head in such a position as to enable the occiput to descend, when the rotation will of necessity ensue. Should we fail in our attempt at rectification by this method, we may succeed by combining the leverage of the vectis with the action of the fingers, passing the blade upwards behind that ear which is turned towards the sacrum. Unless the head is unusually large, the forceps will act only by pulling the forehead still further down, and in all probability making the case worse than it was before.

I append another instructive case from Dr. West's register, as illustrating this point:—

"*Case 5.—Third position—Delivered by Vectis.*—September 27, 1854.—Head descending very unsatisfactorily, and not pressing on lips of os uteri. When it was well dilated and dilatable, I delivered with the vectis. The position was an ordinary presentation of the third kind, with the anterior fontanelle close to left side of symphysis. I introduced the vectis by the left side of perineum, and passing it to the right side of the mother, it fixed readily on the mastoid process over the left ear. As I made traction, the vectis (with the ear) turned to the pubes, and thence to the acetabulum, and presently the head was born. I could still feel the whole of the os uteri when I introduced the vectis. It was flabby and very dilatable, but the head would not descend into the pelvis so as to press on it and pass through it—a good example of bregmato-anterior variety as proved by movements of vectis, and mark of vectis under left ear. *N.B.*—I did not *guide* the head in its descent; I merely made traction, and as it was pulled down it turned of itself, carrying the vectis round with it."

The above remarks, I may repeat, do not apply to those cases in which the head of the child is unusually small or the pelvis of the mother very large, as such cannot be expected to be subjected to the usual laws of the mechanism of parturition. I would remark further, that the descent of the forehead is in all probability more likely to result from the arrest of the advance of the occiput in a large than in a small pelvis, or with a small in preference to a large head, and this for very obvious reasons. The above will, I think, suffice to show that instrumental interference with these cases may be the reverse of beneficial, unless the operator be well assured of the position of the head, and thoroughly understand the mechanism of the process by which nature effects delivery in the ordinary cases of occipito-posterior position.—*Glasgow Medical Journal, Jan.* 1864, *p.* 451.

86.—A CASE OF TWIN LABOUR IN WHICH UTERINE ACTION COMMENCED AND PROGRESSED TO THE SECOND STAGE DURING SLEEP.

By Dr. JAMES PALFREY, Senior Assistant-Physician to the Metropolitan Free Hospital, and Physician to the Surrey Dispensary.

[The patient had borne twins two years and a half before her present confinement. On the present occasion delivery of twins had taken place an hour before Dr. Palfrey's arrival. The children were both alive and rather above the average size of twin children.]

Having attended to my patient, I inquired if she could in any way explain to me the cause of this rapid delivery, when she told me she had retired to bed in her usual health shortly after eleven o'clock the previous night. She soon fell asleep, and believes almost immediately began to dream, the substance of such dream being that she was then in labour, in great pain, and that after some time a fine child was crawling over the bed. She assured me that she continued thus to dream of being in severe labour for a considerable time. After sleeping for a period which could not have been less than four hours, she awoke, and at once discovered some discharge issuing from the vulva. Upon this she aroused her husband, who immediately proceeded to procure a light. To their intense astonishment, however, before the few moments had elapsed which were occupied in getting a light, Mrs. J. screamed with pain, and the first child was instantaneously expelled, the head presenting. A few minutes later labour pains returned, and upon my patient's husband going to her bedside, the feet of a second child were found protruding from the vulva: a third pain expelled the shoulders, and a fourth the head, the placenta following in about ten minutes. To these particulars I may add that this patient had not taken any stimulant or medicine on the previous night.

Now I think there can exist no doubt in our minds as to the fact that in the case just narrated uterine action did actually commence, and that labour did progress to the second stage, during the sleep of this patient. Further, I think we may with equal certainty conclude that during the prolonged dream so circumstantially described by this lady the os uteri had dilated, and the head of the child had passed through and descended to the perineum; and it was at this time that the patient awoke. That she did sleep without interruption for the length of time above stated there can be no question, for she assures me that she has no recollection whatever of waking from the time she went to sleep until a few minutes before the birth of the first child.

I will not stop to offer any remarks upon the many interesting points which will readily occur to every reader on perusing the details of this case. I may, however, in conclusion, add that although I have made diligent search in many of the works on obstetricy and medical jurisprudence, and have also carefully reviewed the notes of many hundreds of cases recorded by my late father-in-law, Dr. Lever, I have been unable to find a case similar to the one I have narrated. I think we may fairly conclude that this case sets at rest a point upon which many of us have hitherto been in doubt—viz., whether or no efficient action of the impregnated uterus can take place during natural sleep. —*Lancet*, *Jan.* 9, 1864, *p.* 36.

87.—RARE CASE OF PRETERNATURAL LABOUR.

By Dr. Henry Munroe, F.L.S., Lecturer on Medical Jurisprudence at the Hull School of Medicine.

[The presentation occurring in the following case must be one extremely rare.]

Elizabeth Richardson, a thin spare woman, poorly fed, and living in a single room in the locality of Witham, was taken in labour of her third child about 5 a.m. on Saturday, February 21st, 1863.

She was attended by a neighbour, who, on this occasino, undertook the office of midwife. The membranes were ruptured, and the liquor amnii escaped about 6 a.m. The labour pains after this somewhat ceased. She had slight pains during the whole day of the 21st; but the child made very little progress. Towards evening, the pains gradually increased; but as night advanced, they began again to decline. The midwife, on Sunday morning, the next day, the 22nd, finding the pains increase, but the progress of the child not at all advanced, or not at all understanding the presentation, sent for Mr. Anningson, surgeon, about 1 p.m. When Mr. Anningson arrived, he found, on examination, that five parts of the child were presenting themselves; viz., one leg was very well down in the vagina, also both arms, the funis, and the crown of the head.

He attempted to turn the child by bringing down the leg, but found that he only wedged the child up tighter, and that all his efforts to deliver in this way were futile. About 2 p.m. he sent for me to assist him. I immediately attended. There could be no difficulty in making out the presentation and the position of the child. I found it quite impossible to return the arms and bring the head into a natural position, on account of the close contact of the uterus over the child.

I at once placed a broad ligature round the child's leg, and gave it to Mr. Anningson. Then pushing up the head and shoulders, and necessarily the arms, I requested Mr. Anningson to make strong traction with the broad ligature. The pains were then not very forcible, for the woman was growing much exhausted. By alternating our movements of pushing up the head and chest and pulling down the leg, our efforts were in about a quarter of an hour rewarded with success by getting the breech into the pelvis low down. The other leg was somewhat in the way; but, by a little manipulation in passing the finger over the thigh, it was also got down. The case now being made a footling case, the child was easily brought into the world alive.

The pressure on the hand and arm of the operator was very great; but the woman recovered without any bad symptom.

How the cord had escaped pressure, I can scarcely tell; but so it was. The child was born alive, and is now in a healthy condition.—*British Medical Journal, Jan.* 2, 1864, *p.* 7.

88.—ON A PLAN OF TREATING PROLAPSE OF THE FUNIS.

By Dr. J. Lumley Earle, Obstetric Surgeon to the Queen's Hospital, Birmingham.

Of all the plans recommended for the treatment of prolapsus funis, it is said that of turning has saved the greatest number of children, and I shall therefore presently compare together the respective merits of turning, and Sir Richard Croft's method, to show how far more likely is his plan to lessen the number of still-births in such cases than is the operation of turning; but, before doing so, it would not be right to leave unmentioned the objections attached to other modes of treating prolapsus funis.

To such methods as pushing the cord above the brim with the fingers, or with instruments, or lodging the cord in the hollow of the neck of the child, there is the objection that the cord, which, when prolapsed, is generally unusually long, is very liable to slip down again at the next uterine contraction. To mechanical means for pushing the cord above the brim, besides the objection already mentioned, there are two others; one is that a medical man cannot be expected to carry about to every labour an instrument intended to remedy an accident which only occurs about once in 221 cases. The whalebone instrument recommended by Dr. Ramsbotham can certainly be made on the spot; but then it has the other objection attached to it, which is, that in order to place a fold of the cord in the loop of the tape, the cord has to be pulled down outside the vulva; this proceeding alone would place the life of the child in a certain amount of jeopardy, and further, by pulling down more of the cord, there would be less chance of its remaining up after being replaced. The plan suggested of placing the cord in a part of the pelvis where it would be the least subject to pressure is all very well in theory, but I should be very sorry to trust to it in practice. It might in some cases succeed where the pelvis was unusually capacious, and the birth premature. The forceps would only be required in those cases where the head was low down in the pelvis, or, at all events, had entered the brim, and of necessity, if the prolapsus has not been remedied before the head has come down so low, the life of the child would be in the greatest of danger, if not lost already.

The operation of turning for prolapsus of the cord (if we only consider it for a moment) is a most unscientific mode of treating that complication. In order to save the child's life, we undertake a treatment, which of itself is but a few degrees less dangerous to the infant. According to Dr. Churchill's statistical tables, the mortality in footling cases is 1 in 2½. On the other hand, Sir R. Croft's method does not alter the natural head presentation, which is the only safe one to the child, but merely replaces the cord in its proper position. His method consists in passing the cord up to the fundus of the uterus enclosed in the hand, and hooking it over the highest portion of the fœtus. In his treatment we do the first step towards turning, that is to say, we pass our hand up to the fundus, but instead of pulling the feet down, we merely hook or place the cord over the highest portion of the fœtus, and then, when an uterine contraction comes on, gradually withdraw our hand. What mode of treating such cases can be more according to nature?

Dr. Churchill, in speaking of this plan, states that it is a very difficult and somewhat dangerous operation, and that he is inclined to agree with Dr. Burns, that "if the hand is to be introduced so far, it is better at once to turn the child." All I can say is, that in the three cases in which I employed this method, I found it a very simple operation, and as to its being better to go on and turn the child when the hand has been passed so far, I have stated the very important reason why it would not be better to resort to turning, as it is in itself a remedy of great danger to the child.

Of course, Sir R. Croft's method would not be practicable in every case more than any one of the other remedies recommended; but in any case in which we could turn we could also employ his plan of treatment. The great point in prolapsus funis is not to rupture the membranes, if possible, until the os is nearly fully dilated, and not to leave the room after the prolapse is discovered, for in our absence the membranes might rupture, and the opportunity for applying proper treatment be entirely lost.

I here subjoin a table of seven cases of prolapsed cord which have come under my own observation, with the methods employed, and the result to the infants.

Methods of Treatment.	Number of cases.	Result to child.	
		Alive.	Dead.
Pushed the cord above the brim . . .	2	1	1
Turned	2	1	1
Took the cord up to the fundus of the uterus, and hooked it over the highest portion of the fœtus	3	3	—

In the last of the three cases treated by Croft's method I subsequently delivered the woman (a primipara) by the low forceps operation, as the head had been in the cavity of the pelvis for some hours, and, notwithstanding the double danger to which the child had been exposed from the prolapsus funis in the first, and the forceps in the second stage, it was born alive. I may mention, although it is hardly necessary to do so, that in all the cases the mothers did well.—*Medical Times and Gazette, Dec.* 19, 1863, *p.* 641.

89.—THE GALACTAGOGUE PROPERTIES OF FARADISATION.

By Dr. Thomas Skinner, Obstetric Physician to the Dispensaries, and to the Female Orphan Asylum, Liverpool.

[The following cases of Dr. Skinner are very interesting. Faradisation has evidently been too little used by us.]

Case 1.—Early in August, 1861, I was consulted by a lady, aged about thirty-six, on account of vaginal irritation and other morbid symptoms, evidently resulting from anæmia. The lady had been lately confined of her fifth child under the care of an experienced accoucheur, and she was at the time suckling her infant. She was ordered moderate doses of tincture of steel thrice daily, but this medication was followed, in a few days, by a complete stoppage of the milk in both breasts. The right breast had never been used for suckling since an attack of mammary abscess some years before, so that I was only required to re-establish the flow of milk in the left breast. At the time of the application, the patient told me that she felt a distinct sensation as of "a rush of milk to the breast." On the 16th, the patient reported, that, *although perfectly incredulous of any good result*, yet within a few hours of the Faradisation a copious supply of milk was permanently established in the left breast. I was then asked by the lady if I could do any good to the breast which had been so long useless as regards the function of lactation. I made the attempt, having little faith in the wished-for result, but I was agreeably disappointed to find that, after two brief sittings of little more than five minutes each, the right breast became as good a "milker" as its fellow. The anæmia was subsequently successfully treated by the *effervescing carbonate of iron*, and with the best effects on the mammary secretion and on the child, considering that effervescing drinks are so much dreaded by nurses and suckling mothers for their bad effects upon the infant. This patient was so much struck with the power of Faradisation in increasing the secretion of milk, the bright idea occurred to her, that "a galvanic machine would not be a bad investment for a dairyman!"

Case 2.—Mrs. —, aged twenty-one, was confined of her first child on April 28th, 1861. Her labour was in every respect natural, and she was seemingly both robust and healthy. Within a month of her delivery, the milk began to decrease and the child became irritable, when artificial food had to be added to the breast-milk; the milk, however, continued *steadily* to lessen more and more, until it was all but entirely stopped in about a week. In this case I could divine no cause for the diminution of the secretion. Good wholesome food, gruel, malt, and the usual stimulants, with open air exercise were resorted to, but all in vain. On the 7th October, 1861, I galvanised both breasts, using the "secondary" current, when the patient complained of a similar "rush of milk" as occurred in the previous case. I told the patient to return next day to have the treatment repeated, but she sent me word that it was not required, as a copious supply of milk had set in shortly after leaving my house. Let me add, that the lady in question, previous to the Faradisation, looked upon the treatment as "a good joke," and was quite as incredulous as the previous patient of any good result.

Case 3.—A strong and healthy young woman, wet-nurse to a patient of my own, from a mental cause, suffered a sudden and all but total loss of breast-milk. One single application of galvanism restored the secretion within a few hours.

Cases 4, 5, and 6.—Were mothers where the supply of milk was always at a minimum, and never altogether sufficient for the child. One or two applications were followed by the desired result in all of them.

Case 7.—Mrs. H., married, and about twenty-three years of age, was confined of her first child six months ago. The breast-milk has been steadily disappearing, although her appetite, digestion, and general system, are perfectly unexceptionable. At present the quantity is so small that she cannot supply her infant with anything like a meal from both breasts, which is a source of great misery to her, as she says, that she "cannot bear to hear the cries of her half-starved babe."

On the 20th December, 1862, I applied the "combined" currents to each breast separately, as more particularly described hereafter. On arriving at home, a distance of about three miles, and not until she applied her child to the breast, did she feel what she termed "the draught." A plentiful meal of milk was drawn off by the child, which went to sleep immediately thereafter, circumstances to which both the lady and her child had been strangers for a month or more.

A good supply of milk remained that night and all next day, when the secretion again became as deficient as before the Faradisation, and the infant began to fall off. Circumstances prevented the lady from returning to me until the 29th December, when the Faradisation was repeated, only I lengthened the duration of the process (about ten minutes to each breast). The result was most satisfactory.

On the 28th January last she called to thank and to inform me that the supply of milk was again abundant on her return home, and I had ample proof in the appearance of the breasts and their secretion, as well as in the look of the child, that the increase in quantity continues permanent.

On the 16th February I received the same favourable report.

Case 8.—M. A., aged twenty-three, wet nurse to a lady in the country, a patient of my own, was suddenly affected by a very great diminution in the supply of her breast-milk. The milk was about nine weeks old and the infant one week. The rapid disappearance of the secretion I believe to have been caused by the [illegible] having told the wet nurse that she observed her milk "failing," and from the fear of losing an excellent situation, the milk disappeared altogether for a whole day.

On the [illegible] February, 1863, when the poor girl was actually trembling through fear of losing her place for want of breast-milk, I galvanised both breasts, by the direct and intramammary plan, by means of the "secondary" current. Like all the other women she felt the current most upon the upper surface of the breasts and towards the axilla, but most when the current was passed direct from the axilla to the nipple. The "rush of milk" occurred for the first time on her way home (within an hour), and it was much greater than she ever before felt. In spite of the strong emotion of fear which seemed almost to overpower her, the quantity of milk in her breasts that night was more abundant than she ever had it, and it still continues so, without having further recourse to galvanism. In this nurse the right breast has always been less developed than the left, but both are now more than sufficient for the child.

These eight cases comprise the whole of my experience of Faradisation as a galactagogue.

Mode of application and the duration of the Faradisation.—To the uninitiated it may be well to state, that by Faradisation is meant the *localising of the galvanic current*, in contradistinction to the all but defunct method of passing the current through distant parts of the body or with the poles widely asunder. Localised galvanism is the discovery of the distinguished Duchenne, who, in honour of our illustrious countryman, Professor Faraday, called the new process "Faradisation."

The instrument which I have always used is the electro-galvanic coil machine, and I have no hesitation in saying that it is of all forms of apparatus the best for the purpose. The current obtained from chemical decomposition is the easiest regulated and applied, the most steady and constant in its action, the least painful and the most effectual. Although I have had no experience of any machine (as a galactagogue) but the one I am about to describe, nevertheless, from my general experience of most forms of electrical and galvanic apparatus, I am much inclined to believe that the magneto-electric, voltaic, and other machines for developing the galvanic current, will effect the purpose, not excepting Pulvermacher's portable and very convenient chain battery of 60 or 120 elements excited by vinegar.

The coil machine which I employ has three currents, namely, the primary, the secondary or induced, and a current composed of the primary and secondary, which I call the combined currents. The instrument is a very powerful one, the battery being composed of from three to six of Smee's cells, and yet it may be made to administer the galvanic stimulus to an infant. It was made to order by Messrs. Horne and Thornthwaite, of Newgate-street, London, and it is impossible to speak too highly of it in every particular. From the above description it will be evident that the machine is the very reverse of portable.

The *modus operandi* which I have adopted in applying Faradisation in the above cases is as follows:

1. *Direct.*—Both poles must terminate in a cylinder into which is inserted a piece of sponge well moistened with tepid water. The positive pole is to be pressed deep into the axilla, whilst the negative is lightly applied to the nipple and the areola; the current should never be stronger than what is pleasant or agreeable to the patient's feelings. Sometimes the nipple is so sensitive that the current can hardly be made too mild. The poles are kept steadily in this position for about two or three minutes, not more.

2. *Intramammary.*—The two poles are then to be pressed into, or, as it were, imbedded in the mamma on each side of the nipple, and to be raised and reimbedded, observing that both poles are raised and reimbedded together. This is to be done all round the nipple and all over the breast, particularly on the upper surface, so as to stimulate, not only the gland, but also the descending superficial branches of the cervical plexus, the thoracic, and the cutaneous branches of the intercostals supplying the gland and its integument. This last part of the process may occupy from two to three minutes (not more), the first sitting. The same is to be done to the other breast, and repeated daily until the secretion is properly established. Hitherto I have never found these means fail to restore the secretion in healthy subjects, in from one to two sittings.

It is well to remember that the Faradisation is not to be applied without intermission until the milk appears in the breasts. The milk we know requires to be *manufactured* from the blood under the direction of a sufficient supply of nervous energy; some time must therefore be allowed to elapse before the result of the Faradisation can be determined. With regard to Case No. 1, I thought that I had failed to restore the secretion, until the next visit of my patient revealed to me the error in my conclusion.

I think it right to remark that I have not tried the effect of the *primary* current alone in any case, but only the secondary and combined currents, both of which seemed to me to be of equal value. I am inclined to believe, however, that the primary current machines will effect the purpose, though perhaps not so speedily or efficiently as machines with a secondary or induced current, because Duchenne has shown that the primary current chiefly stimulates the motor, while the secondary affects more powerfully the sensory nerves—both are likely, however, directly or reflexly, to stimulate the ganglionic, the system regulating secretion.

Remarks.—It may be objected to these cases that the *post* may have been mistaken for the *propter hoc*, and that Faradisation had nothing to do with the results; but the fact that none of the patients believed in the agent, that a few of them laughed at it, that one of them was in *positive dread* of losing her situation through loss of her breast milk, that no other means beyond wholesome diet, exercise in the fresh air, and hygiene, were made use of at the same time with the Faradisation, and finally, the perfect success obtained in establishing a permanent and sufficient supply of breast milk in all of these cases, leaves little doubt in my own mind that the happy results were the direct effect of the Faradisation. Further, let me observe that in Case No. 1, the right breast, which had *lain dormant for years* (the lady having suckled an infant in the interval on the left breast only) was recalled into actual service by two sittings of about five minutes each. In No. 2, where no cause for the *steady* diminution of the milk was observable, and where all the usual means by local heat, friction, warm diluents, stimulants, and starchy food, &c., were tried in vain for a week, yet, immediately after a single sitting, not exceeding ten minutes for both breasts, the secretion was restored in full force as before. The same remarks apply more or less to No. 7. Had the Faradisation failed in Cases No. 2 and 7, a wet nurse was the only alternative.

It has been told me that milk secreted under the influence of the stimulus of the galvanic current is not, or cannot be, healthy milk. Now galvanism, unlike drugs, adds no foreign ingredient to the secretion, it takes nothing from it, nor can it alter it physically, chemically, or vitally, except as regards quantity.

As well might we talk of it altering the lachrymal, salivary, or any other secretion from a healthy gland and system, which it will infallibly increase if carefully and properly applied. *Galvanism seems only to resuscitate the vis à tergo, nature supplies and does all the rest.* But any fear of Faradisation rendering the milk unhealthy or deleterious is entirely set aside by the cases I have narrated—"the proof of the pudding being the eating of it," it will be granted that the proof of the healthiness of the milk must lie in the digestion and assimilation of it; and so far as my experience goes, I found the milk secreted under the influence of galvanism to agree admirably with all the children, the same as if galvanism had never been had recourse to. A medical friend has objected to Faradisation as a galactagogue because he cannot see how a *permanent* supply of breast milk can be the result of one or two applications of galvanism. Now, on this point, the facts which I have stated must speak for themselves, and if my friend or those who are inclined to think like him, cannot or will not accept my statements as facts, then they had better make fresh observations for themselves.

Sources of Fallacy.—So far as sources of fallacy are concerned, I have no doubt there are many, and as there is one which I have already met with, I shall allude to it by narrating the following case:—A married woman, a wet nurse to a patient of my own, before she was a week in service wished to resign her situation, because she said she had very little or no milk for the child. My patient informed me of the state of matters, and I told her there was nothing for it but galvanism. The woman strongly objected, but at last gave her consent. I applied the galvanism *secundum artem*, but next day the nurse stoutly declared she had no milk, and that she could not "*honestly*" stay. In presence of the lady I examined the breasts, and I showed her that they were actually overflowing; and it is my firm conviction that the breasts had never been otherwise. The nurse was most probably suffering from nostalgia, and wanted to go home to her husband and child, whither she ultimately went, and I understand that she has since then suckled the latter the usual time. This source of fallacy cannot be too earnestly borne in mind.

In conclusion, I may state that I was not aware that Faradisation or galvanism had previously been used with success to establish, restore, or increase the secretion of milk. I now find that Dr. Althaus, in his admirable treatise on "Medical Electricity," refers to such cases as having been observed by MM. Aubert and Becquerel. (Althaus, 1859, p. 302.)

Considering the great and sometimes insuperable difficulties attending the management of such cases in practice, I think that we cannot too highly appreciate the value of any safe means likely to prove successful; and although the results of the practice in my hands may seem rather flattering, I feel confident that the same successful results will attend the well directed efforts of others.—*Obstetrical Transactions, Vol. 5, 1864, p. 92.*

90.—ON OVARIOTOMY.

By Dr. Charles Clay, Manchester.

[This interesting paper of Dr. Clay's we cannot abbreviate without omitting some important opinions ; we, therefore, give nearly the whole paper as it appears in the Obstetrical Transactions.]

It is, I believe, well known to many (if not all here present), that for the last twenty years I have been extensively associated with some of the largest operations connected with abdominal surgery, having since the year 1842 made the peritoneal section 116 times ; of which 108 were for the extirpation of diseased ovaries ; 4 where the ovarian tumour was cut down upon, knowing it to be too firmly and extensively adherent to be extirpated, but with the view of breaking up the tumour internally, and establishing an extensive ulceration by seton, all of which succeeded ; once for the Cæsarian operation, and three times for the entire removal of both uterus and ovaries. These operations have been attended by a success far beyond my most sanguine hopes. At first I ventured to expect that about one-third of those operated upon might be saved. The following results will show that I was not disappointed. Of the 108 cases of ovariotomy, 74 completely recovered ; all of them living, and enjoying life and health, for many years afterwards ; many of them having had children, both male and female, and in only two cases did the disease return in the opposite ovary. Of the last two, one died five years after the operation ; the other was so remarkable, that a few particulars will not be out of place, after being operated upon by me, and an ovarian tumour of forty-eight pounds extirpated. On the second day, hemorrhage took place from the pedicle, and I was obliged to remove three of the outside sutures, then grasp the pedicle and apply another ligature, and replace the sutures. This hemorrhage arose from the thickness of pedicle preventing the first ligature from having its due pressure on the supplying vessels, and which had worked itself loose, before a sufficient obliteration of the vessels had taken place. The consequence of this re-application, was a very severe attack of peritoneal inflammation, which was subdued with great difficulty. The case then progressed favourably for some days, when she was secretly indulged (by her nurse) with some buttermilk, which gave rise to violent vomiting and purging, followed by a smart attack of inflammation of the mucous membrane ; when this subsided great prostration took place, and for some time her life was held doubtful. From all these mishaps, however, she recovered, and some months afterwards she married and went to the United States, and there had three children. In 1861, sixteen years after the first operation, the opposite ovary became diseased, and increased to a very large size; she now prepared to return to England, to place herself under my care. In the meantime I wrote, and requested her to consult Mr. Atlee, the principal ovariotomist of America, which she did, and was operated upon for the second time, and to my astonishment recovered. A short time ago, this patient was reported to me as quite restored to health, and capable of attending to the duties of her household. A case like this, in my opinion, speaks volumes in favour of the operation.

To return, however, to the question of the 108 cases (ovarian); 34 only, died; being somewhat short of 30 per cent. Of these 34, 10 died from the immediate consequences of the operation, or from what may be termed shock; 10 died from peritoneal inflammation, generally about the third day; 12 from prostration on or about the sixth or ninth day; and 2 from hemorrhage. The first ten, from shock, were chiefly young females, as well as a greater portion of those forming the second division, viz., those from inflammation. Of those forming the third division (prostration), the majority were elderly, about the period when menstruation was about to cease, or had ceased altogether; and generally about the sixth day. The minority of this section being of the younger class, did not usually die till about the ninth day. Of the two from hemorrhage, one died soon after the operation from the draining of small vessels from a large cut surfice, where extensive adhesions had existed. The other lived thirty hours after the operation, and then, during a frightful dream, she sprang up in bed, whereby the pedicle ligature was loosened and prematurely thrown off, and she sank from internal hemorrhage immediately afterwards.

I will now briefly dwell on a few points in the general management of these cases, which I deem to be of considerable importance, so far as my own experience has enabled me to verify. And first as regards the temperature of the room for operation. I have always steadily persevered in maintaining it to be of very considerable consequence in the successful management of these difficult cases, to have the temperature of the room raised to about 75° Fahr. at the time of the operation; considering that, should any long exposure of the viscera, or of abdominal cavity be unavoidable, the danger may be best met in this manner. I believe such cases so treated are less liable to end in shock. Indeed, I feel convinced, that my own operations owe a considerable portion of their success to this means, and I should be sorry to discontinue the practice. With regard to the

Use of Chloroform,—I am not certain if this agent has really added to the success of ovarian operations. The first fourteen of my cases were undertaken before it was discovered, and of those fourteen, nine recovered. But though I willingly admit the almost impossibility of obtaining the consent of females (at the present time) to submit to so formidable an operation without the aid of this valuable agent; and though I am equally convinced that chloroform is of itself one of the greatest boons to suffering humanity; yet, if it could be accomplished, I should infinitely prefer to operate without it, as the patient would bring to bear on her case a nerve and determination to meet so great a trial, which would assist beyond all value the after-treatment; it would also relieve the case from that most distressing retching and vomiting, so common after all abdominal operations where it is used to the extent that it is required in ovariotomy. And now as to the

Large Incision.—I have always been an advocate for, and practised extirpation by the large incision (that is, commensurate with the size of the tumour to be removed). I have been much censured, and greatly misrepresented, on this point, but after the most serious reflection, I still adhere to the plan, feeling as I do, its many advantages, and believing it to be infinitely superior to any other proposed. My experience sets a much greater value on having a bold and large incision through the integuments, at once affording plenty of room for every manipulation, aided by the eye, than to require a subsequent enlargement, or to drag cysts, or solid masses, through small openings, without a knowledge of what attachments may possibly exist behind, unseen, unfelt by the finger, and the mischief that might arise in consequence of such proceedings. With respect to

Ligatures.—I have for the last twelve or fourteen years, almost invariably used the Indian hemp. In my early operations I used the strongest staymaker's silk, which I discontinued from its aptness to slip. The Indian hemp is well waxed, the finer thread for securing small vessels, and for the interrupted sutures of the integuments; the coarse thread double, sometimes three strands, I make use of for the pedicle, or for the cervix uteri when I have to excise the uterus. I have always found these ligatures answer every purpose well, and do not like to discard an old friend, even for silver, or iron wire, both of which I have tried in other cases, but have not as yet seen advantages enough to induce me to use them as a substitute for the Indian hemp, nor am I an advocate for the clamp.

The Distressing Gulping and Vomiting, in its aggravated form, immediately after the operation, I can scarcely help attributing (in a great measure) to the use of chloroform, since I saw but little of it in the first fourteen of my cases, where it was not used.

I must not, however, confound this with the sickness indicative of peritoneal inflammation. With regard to the first—I have never found, as yet, an efficient remedy; I have tried almost everything I could think of, or my professional friends could suggest, with little or no benefit; and, after all, perhaps a little patience (until the blood has got rid of its load of carbon), and the sipping very small quantities of gum water I have found as efficacious as anything else; taking care to adopt the general rule not to supply the stomach with much food of any kind until its sensitiveness is somewhat allayed, and then in the smallest possible quantities, and of the simplest kind. Again, I have noticed, with some degree of interest, certain periods in the progress of treatment, or

Critical Days.—A knowledge of these periodic changes, their causes, and probable consequences, has led me to watch for them, and be prepared for them whenever they should arise; by this means I have been enabled to meet many difficulties, and contend successfully with them, which, if not carefully provided for, might easily put the case beyond chance of recovery. If the patient does not sink immediately from shock (that is, within the first twenty-four hours after operation), the first critical day will be the third, and the cause of fatality, if the case so terminates, will be unsubdued inflammation. At this place I may remark that I never bleed, but entirely depend on hot fomentations, which are usually sufficient; I prefer this plan, simply because the fatality by prostration is quite enough, without adding to it by depletion in the earlier stages. The next critical period is the sixth day, when I first apprehend danger after the subsidence of peritoneal inflammation (in the elder class of females, particularly), from prostration; should, however, the case be young, this termination may be deferred to the ninth, or next critical day, which is the usual period of prostration for younger females. If the patient passes this point, it assumes a far more favourable prospect for recovery, and the critical days become of less consequence, nevertheless, I have seen the twelfth day usher in some very troublesome symptoms, consequent on the loosening or entirely throwing off the ligatures on the pedicle; and in one or two cases I have seen (about this period) a secondary attack of peritoneal inflammation, or inflammation of the mucoua membrane, which if not carefully and actively managed or foreseen and prevented, may wreck the patient. After this period I generally consider the case moderately safe, and have seldom been disappointed. I have also noticed that the right ovary is more subject to disease than the left; after diagnosing at least 1,600 cases of ovarian disease, my own experience presents as a fact, that one-third more of diseased ovaries are of the right side; physiologically I am unable to explain why.

In Respect to Purgatives.—In the after-treatment of ovarian operations, I never advise them, but always depend, as much as possible, on ox gall, internally, backed by the use of enemas, which are either of simple gruel, with castor oil, or, if the unloading of the bowels be more urgently required, of one or two ounces of the recent ox gall added to the gruel. The inspissated gall, in form of pill, acts gently without purging, by simply keeping the fæcal matter in a more soluble condition, and requires less effort to expel it. This is of great value at a time when the liver is very deficient in its action. It has also another beneficial tendency—the prevention of flatus. In fact, it is a substitute for healthy bilious secretion, at a time when it is generally deficient or wanting. The enema is at all times sufficient to unload the bowels, without straining, for I never allow the patient to empty either bladder or rectum without the assistance of catheter and enema for the first five or six days after operation, believing such efforts only disturb the healing process of the integuments, and so retard recovery, more particularly if flatus be allowed to accumulate in the bowels.

On the Operation in Respect to Age.—I have operated at all ages, from sixteen to fifty-seven; and have been pretty equally successful at most of the ages included, no age particularly presenting itself more successful than others. If the majority of young females die from shock, or from inflammation, it is equally apparent the majority at advanced ages die from prostration. Still, had I the choice, I should prefer that period of life for the operation when menstruation is about to cease or had altogether ceased. Such cases (if not worn down or too much emaciated by the disease previously), I think, have the best chance of success, are less liable to inflammation, and, I believe, bear the operation better, being more disposed to submit, and less impatient under the restraint and self-denial so necessary in such cases. Having made these observations, I may remark that I believe I have been mainly instrumental in bringing ovariotomy before the public, although I have had much to contend with, and have often been grossly misrepresented; still, in spite of every opposition, I have lived to see it established (under proper circumstances) as a legitimate operation in surgery, and practised by others with a fair share of success. Had I never accomplished any other great object in my professional career than this, I still fancy I shall not have lived altogether in vain, but have contributed something to the benefit of my species. To all who have the slightest idea of the magnitude and uncertainties of this operation, the incessant care, and constant attention required subsequently to conduct such cases to a successful issue—*such* will readily admit that a rate of rather more than 70 per cent. of recoveries is a victory in modern surgical art worth contending for. I am also impressed with the belief that this operation is not a very suitable one to offer a great prospect of success in large hospitals. The constant, almost indefatigable attention for a considerable time will always be best provided by a private operator, whose credit and interest are at stake, and who spares neither time, trouble, nor inconvenience to accomplish success. Perhaps no persons not so disposed should have anything to do with extensive and important operations, particularly those of the female abdomen. In addition to these operations, I have once made the Cæsarian section with partial success, the patient living to the fifteenth day, but the poor object being in the last stage of phthisis, sank more from that disease than the effects of the operation.

Lastly, I had so far back as in 1844, extirpated both uterus and ovaries. These cases were reported in the Medical Times of that year as unsuccessful operations. It will be here necessary to give a short outline of one of these cases.

A. Brooks, of Staleybridge, aged fifty-two. Had an enormously enlarged abdomen; the disease had existed sixteen years. The swelling did not occupy more of one side than the other, or at least the preponderance was so trifling that it could not be stated with any certainty. Menstruation had always been irregular, but never suppressed; there was much greater density on the left side; it was therefore considered by my friends and myself as ovarian. I therefore operated on January 16th, 1844. Immediately after the first incision, about twelve inches in length, it was evident that extensive disease existed, independent of the left ovarium, which was enlarged to about four pounds in weight. The uterus itself was enlarged to twenty pounds in weight, and the cystic deposit amounted to about eight pounds, making thirty-two pounds in all. Having proceeded so far, the question arose, what was to be done? I determined to extirpate the whole, converting the vagina into a *cul-de-sac*. The operation was soon and easily accomplished? inflammation set in and was subdued; subsequent depression and exhaustion supervened, from which she rallied. On the twelfth day she was doing so well that every reasonable hope was entertained of her ultimate recovery; a nutritious diet had been given, and everything promised well. From the fact of both ovaries and uterus having been extirpated, my interest was [illegible] excited. On the thirteenth day the nurse put an end to all the flattering prospect by an accident. On lifting her from the bed to ease the bedding, the patient fell on the floor somewhat violently, although three women had hold of her: the result was, inflammatory action arose, and though every assistance was rendered, she died on the morning of the fifteenth day. The result was most mortifying, for had not the accident happened, I feel assured the case was doing so well that a recovery might have been confidently anticipated. The length of time from the operation, and the difficulties already overcome, fully entitle me to think so. This case, of course, will be used statistically against the operation of ovariotomy, although in reality it was a most successful one as far as the operation was concerned, and was, in fact, the first operation in the world where both uterus and ovaries were extirpated through the abdominal walls. Reflecting on this case, it appeared to me quite possible for a female to recover with the uterus extirpated. I had frequently extirpated one ovary successfully, I had also successfully removed both ovaries; and in the case just given both the uterus and ovaries had been taken away, the patient lived to the fifteenth day, and in all probability would have entirely recovored but for the accident.

I therefore determined in my own mind on the extirpation of the uterus and its appendages, if ever a favorable opportunity presented. I was nearly attempting it three years ago in a case brought to me by Dr. West, of this society.

The third case, which presented itself to me in January, 1858, I now give in detail:—

Miss N., of S—, consulted me respecting a large abdominal tumour; she had complained of uneasy symptoms for some years previously. In other respects quite healthy, was but little emaciated; menstrual periods had always been quite regular, the tumour was largest on the right side, very solid, irregular in its form, intensely hard, easily moveable, indicating no adhesions of any consequence. Its weight at this time appeared to be five or six pounds. She complained of weight and feeling of bearing down. On making a vaginal examination, I found the tumour filling up a large portion of the pelvic basin, and it was with difficulty the finger could pass along the sacrum, and still greater in trying to reach the os uteri, in the direction of the pubes, behind which I supposed the os uteri was to be found, as there was no trace of it in any other direction. Up to this period there had been no difficulty in passing urine; no obstruction to menstruation, but it was always necessary to use an enema to secure a movement from the bowels, and then the forced fæcal matter was flattened like tape. As the tumour had not apparently increased in size for the last few months, I advised her to wait, that its progress might be watched; but if the difficulties in voiding urine or motions increased, to visit me again. My first impression of this case was that it was uterine, although it in many points resembled ovarian; my inability to reach the os added to the difficulty, but the intense hardness was only such as is felt in uterine fibrous masses, whilst the irregular lobulated form and its mobility indicated an ovarian character; one point, however, it is necessary to state, that whilst the pelvic mass was immovable, it was not in the least affected by the mobility of the abdominal portion. In 1862 she visited me two or three times; the main features of the case continued much the same, except that she appeared more emaciated, and the filling up of the pelvis was now so nearly complete, that I apprehended very soon an entire stoppage would occur, and inability to discharge the contents of the bowels and bladder would force me to some step for relief. It was impossible to pass the finger along the curve of the sacrum without injuring the perineum, and by the pubes it was impossible to pass even a bougie. I therefore fixed the 2nd of January, 1863, for the operation, stipulating that, after making an exploratory incision, I should be at liberty to forego the extirpation if any peculiar difficulties presented themselves. On the day stated, in presence of Dr. Hammond, of Preston, Mr. Carr, of Sheffield, and Mr. Edlin, surgeons (the room being previously heated to 74° Fahr.), I made an incision of about eleven inches, when a large uterine tumour came in view, of very irregular shape, one portion of which was firmly embedded in the pelvis; the upper and larger portion (that could be easily moved from side to side) was quite free from any but its natural attachments, and I hoped the pelvic portion would prove also free, but though there were no adhesions, it was with the greatest difficulty it could be dislodged from the pelvic cavity, and it required considerable force to effect it. The ovaries on both sides were close to the uterine tumour, the broad ligaments being entirely involved in it, and forming a partial covering to the mass. At the posterior part of the tumour were three nodules or .excrescences, about the size of small eggs, indicating beyond all doubt the fibroid character of the disease. The right ovary was diseased, but not much enlarged; the left ovary about the natural size, but discoloured; and the surface of the tumour, where it was covered by the broad ligaments, was freely injected with vessels. I dissected the broad ligaments from the tumour, and securing them by ligature divided them, and continued my progress down to the cervix. Having satisfied myself by passing my finger well round it, I placed a ligature on it just immediately above the plane of the os, consisting of three strands of strong Indian hemp, and then divided the cervix. My intention in so doing was to secure the os itself for the summit of the vaginal canal, and to keep entire the vagina in its full integrity, rather than run the risk, as I had done in the case of 1844, of gathering together within the ligature the vaginal coats, just below the os; in that case, if it had recovered, I should have had a short and contracted vaginal canal, with less certainty of healing. In the present case I placed full reliance on the powers of nature, by the help of the ligature, to obliterate the passage through the os, and prevent the admission of air (so much feared by many) within the abdominal cavity. The tumour was removed with very trifling loss of blood; the integuments were brought together and secured by six interrupted sutures, padded, bandaged, and the patient placed comfortably in bed in about fifteen minutes from the commencement. Three grains of solid opium were given.

Immediately after the operation, Prof. Simpson arrived unexpectedly from Edinburgh, and carefully examined the tumour as well as the patient, in which he felt great interest. At his special request the tumour was forwarded to Edinburgh for more minute investigation. I have since received from him the following remarks, which I have extracted from his letter, dated January 21st, 1863 :—

"I have repeatedly and carefully examined the mass of fibroids which you extirpated. The os uteri is not included in it; but the cervix uteri appears to have been divided and cut through immediately above the plane of the os. The uterine cavity is dilated and elongated, and contains a semi-pediculated or polypoid fibroid of the size of a small walnut. The whole mass weighs eleven pounds; one ovary and fallopian tube is attached to it, the other cut through in the course of its length. The whole mass then consists of the entire body and the cervix up to the os, with a large mass of fibroid tumours growing in their walls.

"I have had a drawing made for a woodcut, of which I will soon send you a copy, intending to illustrate the position and form of fibroids, and the changes produced by them in the uterine cavity.

"Your case may turn out as a precedent for operative interference in some exceptional cases of large fibroids of the uterus, and I congratulate you most sincerely on the happy recovery of your patient."

I need not occupy your time with any further description of the tumour after this letter, except to remark there was but little vascularity in the mass itself; but the broad ligaments seemed more freely supplied with blood-vessels than is usual. I need not occupy much of your time in the after treatment of this case, being very similar to that of ovarian extirpation. For the first twenty-four hours the retching and vomiting were extremely troublesome, but she had occasional short sleeps from the opiate, which was renewed to the extent from eight to nine grains in the first day and night. The second, third, and fourth days were entirely occupied in subduing peritoneal inflammation, which had ceased by the end of the fourth day. The secretion of the urine was free from the first, and the bladder was often emptied by catheter; after the fourth day it was more than usually copious (which I always esteem a favourable sign). On the fourth day the bowels were relieved by enema, and part of the interrupted sutures removed (I generally remove them on the third day); but in this case one or two of the sutures had broken through the skin already, and I thought it best to keep the remainder in a little longer than usual; the abdomen was now assuming its natural size, but on the sixth day it again became distended, and I feared some further inflammatory action, and active fomentations were resumed with good effect. On the eighth day beef-tea and isinglass were taken and relished, which was now necessary, as some indications of prostration beginning and increasing the following day (which was the ninth), so as to create some alarm for her safety; she became also restless, and required opiates. On the tenth day she had passed a good night, and was considerably better; eleventh and twelfth days, still improving, the bowels acting regularly, occasional natural sleep, and a desire for food. On the fourteenth day pus was freely discharged along the ligatures, showing the work of reparation was progressing favourably. The urine was loaded with a dark sediment that had a very fœtid smell, and continued in this condition for some days after. On the fifteenth day two of the ligatures came away, the pus decreased, the urine was less fœtid. Sixteenth, seventeenth, and eighteenth days were marked by general improvement; all the ligatures except that on the cervix had come away; this latter still felt firmly adherent, and I was not in the least desirous to use traction. I therefore determined to allow it time to release itself. Nineteenth day, food taken with quite a relish, consisting of mutton chop, oysters, poached eggs, milk and light puddings, porter, and occasionally a little wine. Twenty-first and twenty-second days she sat up (two hours at at a time). From this time her progress was that of continued improvement; occasionally her nights were rather restless, from the amount of food the stomach was called upon to digest; disliking the constipating effects of usual opiates, I gave thirty-five drops of Napenthæ as a sedative, and found it answer every purpose without confining the bowels. On the completion of the thirtieth day from the operation I still found the ligature attached to the cervix, but somewhat looser, but as there was no wound except the small aperture through which the ligature came out (and that no inconvenience) I let it remain.

On the 6th of February, 1863, and thirty-five days from the operation, my patient returned to her friends, and bore a railway journey of nearly forty miles, without any inconvenience or apparent fatigue.

This, I believe, is the first operation of its kind established as a cure in the British dominions; *that is, the extirpation of the uterus and its appendages entire, through the walls of the abdomen.* The cases in 1844 were (as far as the operation and their immediate consequences were concerned) decidedly successful, and, but for an accident as unforeseen as mortifying, a recovery was all but certain.

I would just add in conclusion, that in four cases of ovarian cysts—injected by strong tincture of iodine—*three* have entirely recovered; one died from inflammation; two of the cures, though some years ago, show no signs of the disease returning. —*Obstetrical Transactions, Vol.* 5, 1864, *p.* 58.

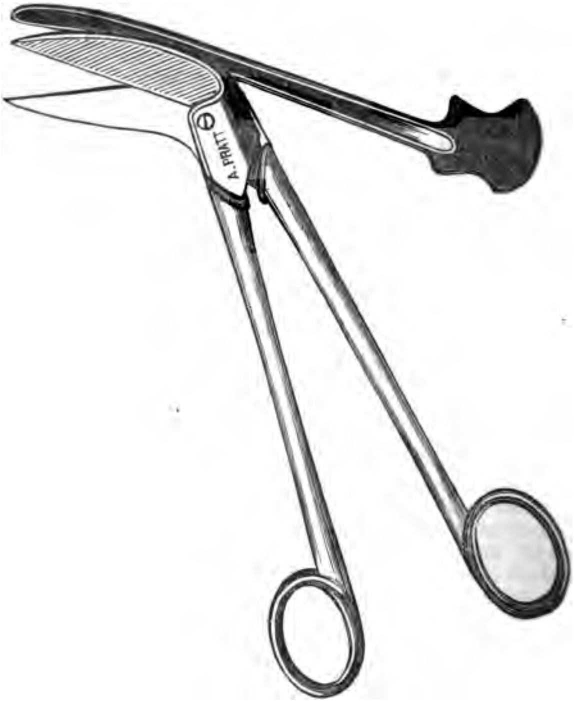
91.—ON THE TREATMENT OF OVARIAN DROPSY.

By I. Baker Brown, Esq.

[Until recently, Mr. Brown along with most of the profession, considered that a unilocular cyst springs always from the Fallopian tube, and a multilocular one always from the ovary. Within the last few days, however, he has met with a large multilocular cyst springing entirely from the broad ligament, and quite unconnected with the ovary. In the case of a unilocular cyst Mr. Brown advocates the plan of tapping and pressure, before attempting extirpation. To a successful termination of a case it is essential that there be no breaking down of the vital powers. We have no right to expect a happy result where the vital power is low or exhausted. Mr. Brown proceeds :]

I will now suppose that it has been agreed that a case shall operated upon. What then, gentlemen, is the next step? It is to prepare the patient. On this I lay very great stress. It is highly important that the operator should not only get the bowels cleared out and see that the kidneys are secreting healthily, but he should be especially careful that the skin is acting favourably and well. To secure this action the patient should have three or four warm baths in the course of a week or ten days. It will depend very much upon the action of the skin whether or not there is effusion into the peritoneum. I also give my patients steel in combination with tincture of arnica. Having got the patient perfectly prepared, and being careful to operate a week after the menstrual period, so that all the uterine organs may be in as quiet a state as possible, the next thing to consider is the position of the patient. There are various plans adopted, and each operator seems to follow his own; but in this, as in many other matters, little deviations make no difference in the real operation. I always place the patient on her back, with her legs protruding over the edge of the table, her head supported by a pillow; an assistant holds each leg widely apart from the other, and I stand between the legs. Under these circumstances the operation is one of the simplest in surgery. There is no difficulty in performing it. It requires merely a cool head, a steady hand, and a ready adaptation of means to ends. An incision is made of about two or three inches between the umbilicus and pubes. Two or three sweeps with the knife are made until the skin, fat, and muscles are divided. (Here I would give a caution. I used formerly not even to employ a director, but to cut down, carefully dissecting, quite through until I came to the cyst. The rapidity with which the operation was thus effected was tempting. One day, however, I happened to be in a more cautious mode than usual; I used my director, and came to a semi-transparent body lying just in the centre of the wound which I had made. It did not look like a cyst, and on examination I found it to be a knuckle of intestine, which had got fastened up, and which adhered to the cyst between the parietes and the tumour. It was easily detached, the adhesion not being of long standing, and the operation was proceeded with; but if I had gone on in my usual way, I might have cut into the bowel and so killed my patient.) Having dissected down to the peritoneum, a small opening is made, and a large broad-grooved director introduced; then, instead of using a knife, the point of which might do mischief, I employ a pair of scissors (Fig. 1). As the point is turned outwards, no possible harm can be done. The tumour is then seized with a pair of vulsellum forceps, which are held by an assistant or the operator. A large trocar is introduced into the cyst, and the fluid allowed to pass off as fast as practicable; and if there be more than one cyst, each is tapped until the mass is sufficiently reduced in size to come out of the opening which has been made. If it is found that the mass cannot be got through this opening, the incision should be extended upwards beyond the umbilicus, unless the cysts can be further tapped and so reduced sufficiently. Frequently a second pair of forceps will be required to hold the mass. If there be adhesions, they must be broken down or tied. If the adhesions be to the omentum, they must be first tied with silver wire and then cut off anterior to the ligature. (In one case, the whole of the omentum being adherent, I tied eight separate portions in this way, and in no one instance have I seen any ill result from such a course). Having thus removed the adhesions, an attempt must be made to withdraw the mass. If it does not come out, it is certain that there are adhesions underneath in the pelvis, or on the pelvic rim. These adhesions must be traced out and broken down, if possible; if this cannot be done they must be separated, and tied with silver wire or twine. If with silver wire, let the ligatures remain in the cavity of the abdomen; if with twine, my plan is to bring the ligature outside. The next step is to bring the tumour out, and put it in a basin. It is then seen to which ovary the growth

FIG. 1.



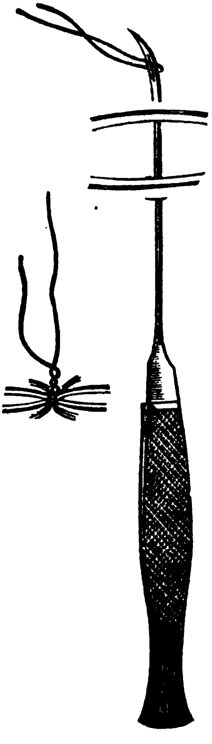
is attached, and whether the pedicle of the tumour is long or short. At this stage of the operation different plans are adopted by different ovariotomists. Dr. Clay uses a ligature of Indian hemp, and returns the tied pedicle into the cavity of the abdomen. Dr. Tyler Smith has latterly followed a practice first adopted by Dr. S. Rogers, in New York, in 1829, afterwards by Dr. Billinger, of the same city, in 1835, and by Dr. Siebold, of Darmstadt, in 1846—viz., of tying the pedicle, cutting the ligature off short, removing the tumour, and closing the whole wound at once. All these gentlemen have been successful. Dr. Tyler Smith has had seven or eight successful cases; and I hear that Mr. Fergusson has lately adopted this plan of procedure with success. If experience confirm these happy results, then certainly this plan will supersede both the twine or silk ligature and the clamp, which I and others generally use; for, as a rule, I employ a common clamp resembling a pair of carpenter's callipers, a little improved by our instrument maker, Mr. Pratt. It is a somewhat lighter instrument, but the principle is the same. The tumour is cut off close, and the clamp remains outside the abdomen. It happens sometimes that the pedicle is so broad that no clamp can grasp it, or the adhesions may extend from the pedicle, running up one side and occupying half the pelvic rim. We had a case of that description here, and we had to take up four different pieces. We had only three clamps; these were used, and the fourth piece was tied with Indian hemp. The patient made a capital recovery.

It is sometimes found that both ovaries are diseased, and the second should always be examined for the purpose of ascertaining whether such be the case or not. If the second be diseased, and cysts are found growing from it, it is better, if they are small, to cut them open down to the base, and then tie; or, if there are many of them, to tie or clamp (you cannot generally clamp) the ovary, and cut it off. Sometimes the second ovary will have as distinct and large a growth as the first that was removed. I have seen two instances in private practice where this was the case. Both patients did well. In the last operation we had in this institution both ovaries were diseased, the second less than the first; both had multilocular tumours. These tumours were secured by clamps in the way I have recommended, and this patient also made a good recovery.

Having so far succeeded, the next step is to remove any fluids or blood which may have escaped into the abdomen. I should have mentioned before, that to avoid the escape of blood from the cut wound, an assistant is required on each side with warm flannels in their hands. They grasp the edges of the wound, and hold them everted as the incision is made. If the wound bleed, the blood comes into the flannel; and if the intestines rise up, the flannels keep them back. There is, perhaps, no operation in surgery where more depends upon good assistants. But even after using the flannels, blood or fluid will sometimes escape into the cavity of the abdomen. And here again there is a diversity of opinion as to practice. Some recommend sponging out freely from the cavity every particle of blood or fluid, and those gentlemen who advise this course have had great success; but I have myself, in more than one instance, seen the sponge do positive mischief. If the experiment be made of putting a piece of sponge into the cavity of the abdomen, letting it rest upon the peritoneum, it will be seen how soon the sponge will stick so firmly that it can hardly be torn off. In a case I once saw, I observed this danger. I saw that the sponge irritated the peritoneal coat very much, and therefore determined not to use it. I now use new, warm, Welsh flannel, and remove every clot of *blood* I can find; but with regard to simple fluid escaping from the cyst, unless it be of a purulent or acrid nature, I do not lay great stress upon getting it all out, and certainly the results have in my hands been just as good as where sponging has been employed. It has also happened, on more than one occasion where sponge has been used, that a piece of it has been left in the body, and has there remained undiscovered until the post-mortem examination disclosed it.

The next thing is the closing of the wound. Much has been said and much written upon this subject. Some have used hair-pins; some, silver sutures; some, three or four deep twine sutures, besides several interrupted sutures. Some lay great stress on enclosing the peritoneum; others say that the peritoneum is of secondary importance. Now it appears to me that the sutures should be placed as deep as the edges of the wound, so that every portion of divided tissue may be brought exactly in apposition. For this purpose it is not necessary to take up any of the peritoneum. The simplest way of passing the sutures is to have a needle like that represented in Fig. 2, which I had made on purpose. It is passed through the first side, and then the second; then the assistant threads the needle, and by simply twisting it back, withdraws it. Unfasten the wire suture, and the thing is done. By a simple twist of the wire three times upon itself the edges of the wound are brought perfectly together. The sutures are placed about half an inch distant from each other. When four, five, or six have been put in, the parts will be perfectly in apposition down to within an inch of the clamp. Here I come to another practical point. When the clamp is neared, the wound should not be closed exactly, because if it be so the pedicle will grow to the cut edges; and when the clamp is removed—which should be done in two, three, or four days, as soon as the parts of the pedicle are thoroughly dead—it will be found that the pedicle will be so adherent to the edges of the opening that it will not recede and fall within the cavity of the pelvis. This, again, I learned from experience in this institution, in two cases where I clamped, and kept the clamp on some six or seven days. The patients made good recoveries, but when menstruation came on, the skin broke over the stump, and there was vicarious menstruation every month. Believing this to be not only a most disagreeable but an undesirable result, I now leave a sufficient space between the clamp and the wound, so that when the clamp is taken off, the pedicle must fall in and be covered up by the subsequent union of the two edges, which are brought together by straps of plaster. I understand that this vicarious menstruation has been disbelieved in this city by some eminent physicians, but it is a fact which is well attested. I have recorded it in my book on Ovarian Dropsy, and it can be proved by the evidence of many gentlemen who have visited this institution. One lady on whom I operated frequently visited me, complaining of the great discomfort of this abnormal menstruation. This was four or five years ago. She is now pregnant for the first time in her life, and her pain is very great from the stump, which is thinly covered with skin. It will be seen, then, that the hint which I have just given is important.

FIG. 2.



The sutures having been placed, the wound is covered with oiled or wet lint; then a many-tailed flannel bandage is put on. I have used this bandage for many years, and it has the advantage of being extremely easy of application. If the patient is bandaged with an ordinary flannel bandage, it is very irksome and detrimental. This one is made with two loops to enable it to be used larger or smaller. The string on one side is brought through the loop on the opposite side; and the string on that side which comes from the root of the loop is passed through the one on the opposite side. By this means the parts are brought perfectly in apposition, and the strings are tied in a bow. The advantages of this bandage are greater than at first appear. Tympanitis often follows the operation; and if the bandage is not such that it can be removed and altered at pleasure, the patient will suffer great discomfort. Now the nurse or surgeon in attendance, finding tightness or distension, can loosen this bandage more or less as may be required; and when the wound requires dressing or looking at, the strings have but to be untied.—*Lancet, Feb.* 13, 1864, *p.* 177.

92.—SYPHON-TROCHAR AND HOOKED CANULA FOR OVARIOTOMY.

By T. Spençer Wells, Esq.

[Mr. Wells has been so often written to by his friends for an account of the canula used by him in ovariotomy that he gives an account of the instrument. It is a syphon-trochar, having an india-rubber ball fixed three or four inches from the extremity, and a pair of self-applying hooks between this and the point. The instrument may be obtained from Weiss. The following advantages are gained by its use :]

1. Instantaneous escape of the contents after the cyst has been punctured. This is of great consequence, as the short time occupied in withdrawing a solid trochar from the canula may permit some escape of fluid from a tense cyst between the cyst and canula, and lead to an enlargement of the opening.
2. Complete protection (by withdrawing the point) from injuring or piercing the cyst involuntarily.
3. Secure compression of the opening in the cyst against the canula, and a safe mode of drawing the emptied cyst from the abdomen by one instrument.
4. A convenient mode of carrying off the fluid without wetting the patient's clothes or bedding, and (by means of the india-rubber ball which forms part of the tube) of exciting some force of suction and ejection, and so cleaning the canula or tube of any obstruction.—*British Medical Journal, Dec.* 12, 1863, *p.* 651.

93.—CASE OF ENLARGED OVARY; TAPPING THROUGH THE VAGINAL WALL; RECOVERY.

By Dr. J. Brisbane, Galashiels.

[The patient suffered from various symptoms indicating ovarian disease up to the period which we select as the commencement of our reprint of the case.]

At this time, on examination per vaginam, a smooth globular tumour, somewhat larger than a walnut, situated behind and to the left of the uterus, was felt. On examination per rectum it was felt more distinctly. From the pressure of this upon the rectum, great difficulty and pain were experienced on defecation; nor indeed could a stool be passed at all unless it were liquid. It was also found almost impossible to throw up enemata, the nurse stating that there was some obstruction which forced back the fluid as fast as she pumped it up. On touching this swelling, either per rectum or vaginam, the patient recognised it as the centre of all her sufferings.

Assisted by Dr. Brown, I proceeded to tap. Before plunging the trocar into the swelling, I suggested that one of Dr. Alex. Wood's syringes should be introduced as a sort of *exploratory* puncture. The uterine sound being introduced, and the uterus held aside by Dr. Brown, I introduced, guided by the index finger, the nozzle of the small syringe, and plunged it into the tumour. By elevating the piston I thus filled the syringe, and removed ʒi. of clear amber-coloured fluid. It was thus introduced several times; and had the nozzle of the syringe been of sufficient length, so that I could have unscrewed it externally to the vagina, I could thus have emptied the cyst almost entirely. As, however, the screw of the syringe was situated considerably within the vaginal orifice, so that it was very difficult and almost impossible to screw and rescrew it so often, I plunged a fine trocar into it, and allowed the greater portion of the fluid to dribble away. The fluid thus drawn off was of a clear amber colour, perfectly homogeneous, and coagulated immediately into a stiff tenacious jelly. It was evidently an inflammatory product. I have no doubt that a chronic inflammatory action had been set up in the left ovary, consequent on the violent purging to which she subjected herself after her first illness; at least she dates her ailment from that time, and it had, no doubt, with occasional remissions, been going on up to the time when it reached its acme of intensity immediately before the operation.

The patient was much relieved. Defecation, which formerly had been extremely difficult and painful, was now comparatively easy. An enema, which she had the following day, passed up without difficulty, and there was no forcing back, such as was formerly experienced. The severe burning pain which has annoyed her so much materially abated, and the general symptoms gradually improved. A discharge of fluid continued for some time after this, together with an *uneasiness* (she stated she could hardly call it pain) in the left iliac region. A succession of blisters and a change to the seaside have done much to remove this; and upon the whole, she feels as strong and healthy as she ever did, and more so than she had been for some years past.

Regarding this operation, I would remark that the use of Dr. A. Wood's syringe, in the manner above mentioned, is, so far as I am aware, novel. I have never heard of its having been before applied in the capacity of an exploring needle. Its great superiority, however, over the ordinary needle is sufficiently apparent, when fluid contents require to be examined, as a considerable quantity can thus be obtained for examination. Small cysts, such as ganglia, &c., might also be emptied in this way, and afterwards, if it was thought necessary, injected with iodine.—*Edinburgh Medical Journal, Dec.* 1863, *p.* 548.

94.—ON THE APPLICATION OF ACUPRESSURE IN THE OPERATION OF OVARIOTOMY.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

It has been hitherto a difficult problem to obstruct the vessels of the ovarian ligament in ovariotomy without leaving a foreign body upon the stalk of the tumour, either to ulcerate or to slough through it. The stump of the ovarian pedicle—in cases where the pedicle is ligatured *en masse*—forms necessarily a dangerous element against the success of the operation; for it is allowed to pass backwards into the cavity of the peritoneum so strangulated as inevitably to produce ulceration or mortification in the ligatured part. Again, when the clamp is used, the straining and drag upon a shortened pedicle is sometimes so great as to endanger the life of the patient, on the same principle as a piece of stretched omentum in a strangulated hernia does so. Besides, the pedicle when clamped and dragged out externally prevents the lower part of the wound from healing until the clamp is removed and the pedicle retracts. It sometimes is long in sloughing through the strangulated pedicle. In a case of ovariotomy which I performed last year, the clamp was twelve or thirteen days before it separated. The patient made a perfect recovery, but there was an umbilical-like wound in the abdominal walls from this effect of the clamp, which was long of healing. I left the clamp on in this case thus long in order to see the length of time it would take to ulcerate through the pedicle, particularly as there were no symptoms urging its removal. When removed early, that is, on the second or third day, the pedicle is apt to become retracted with a piece of dead and compressed tissue attached to it. In the Medical Times for March, 1860, page 285, I ventured to suggest a mode of applying acupressure to the ovarian stalk in cases where ovarian disease removed by extirpation, but I am not aware that that method has yet been tried. In the following case of ovariotomy I lately applied acupressure successfully in another and a different way.

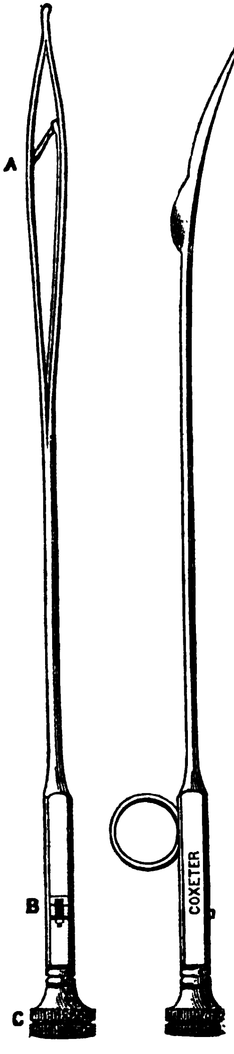
Case 21.—Ovariotomy—Acupressure applied to the Stalk, and removed in Forty-six Hours.—The patient was married, the mother of one child, and had been three times tapped. The multilocular ovarian tumour weighed upwards of 30 ℔s. The largest cyst contained 23 ℔s. of pure pus. The tumour was universally adherent, in front, to the abdominal walls. After detaching it, and having drawn it out externally, I applied a clamp to the ovarian stalk so near the tumour as to include a portion of it. After the tumour was removed, the uterus and attached parts showed a strong tendency to pull the clamp downwards to the pelvic brim. It was evident that it could not be left on without great and inevitable dragging upon the pedicle. I then passed a glass-headed needle, two and a-half inches long, twice through the ovarian stalk, immediately below the clamp. In passing it thus twice, I left out at either side a portion of tissue, to which no compression was to be applied, with the hope that it would supply with blood and vitality the strangulated stump of the ovarian stalk. The needle was thus placed under nearly the same conditions as in the method of acupressure which I have previously described, and which, let me add, I believe practitioners will find the simplest and most serviceable of all the forms described for common wounds. To compress the stalk sufficiently, I threw over the point-end of the needle a loop of strong tinned iron wire of the size described by iron-workers as No. 22, twisted it half round the headed end of the acupressure needle, and then drew it with sufficient tightness to prevent any discharge of blood from the included vessels. A slender cylindrical iron hood, three-quarters of an inch in length, was placed over the sharp end of the needle to prevent injury and lesions from it; and this hood was fixed to the head-end of the acupressure needle by a duplicature of slender suture wire of the size No. 32. The clamp was then removed; the small remaining portion of the tumour which it embraced was excised; and the acupressure needle was carefully placed across the brim of the pelvis. A string attached to the beaded head of the needle and the wires used in compressing the stalk were left out at the lower edge of the abdominal wound. The wound itself was united by stitches of iron thread, passed so as to embrace the peritoneum, except in one instance, where the stitch was only superficial. The wire threads were carefully moulded to the exact shape of the parts in which they were each respectively placed, by pressing each in succession with two fingers, one applied inside and one out. The acupressure apparatus was removed in forty-six hours by merely pulling at the head of the acupressure pin. The two sets of wires, one of them carrying the hood, came out with it. Four days after the operation, all the deep iron stitches, without a drop of pus following any of them, were removed, and the wound was found healed from end to end by the first intention. The superficial iron stitch was left in a few days longer, and two or three drops of purulent matter followed its withdrawal. Thirteen days after the operation, the cicatrix at its uppermost end opened under a fit of coughing, and a quantity of pus which had been forming a small swelling there for a few days escaped. In this instance there existed before the operation the purulent diathesis in the most marked form; the patient's pulse was never for weeks under 100, and usually considerably higher, and there was a purulent collection of 23 ℔s. of matter within her body. Since the small abscess in the abdominal walls has given way, the pulse has sunk towards the natural standard, and she is rapidly regaining health and strength.

Probably, it will turn out, that allowing the tied ovarian pedicle to pass back into its normal situation under the compression of an acupressure needle, and removing that needle in a day or two after its application, will diminish the risks and mortality attendant upon ovariotomy. I am the more inclined to take this view, from seeing it stated, in the last edition of Dr. Churchill's very able work on the Diseases of Women, that my friend, Dr. Tyler Smith, of London, has only lost three out of eighteen cases of ovariotomy; and the principal peculiarity, I believe in Dr. Smith's mode of operating consists in allowing the stalk of the tumour, tied with an organic ligature of silk or cord cut off short, to slip back at once into the cavity of the abdomen. In this method we encounter the dangers occurring from a foreign body, such as a ligature being left permanently within the cavity of the abdomen, and from the tied end of the stalk being ulcerated and sphacelated by that ligature. In acupressure we avoid both of these dangers.—*Medical Times and Gazette, Feb.* 6, 1864, p. 143.

95.—AN INSTRUMENT FOR DILATING THE CANAL OF THE CERVIX UTERI.

By Dr. Wm. O. Priestley, Professor of Obstetric Medicine in King's College, London.

Mr. Coxeter, of Grafton-street, has recently made, under my direction, a dilator for the os and cervix uteri, which I have found very convenient and effective, and which may be



serviceable in the hands of other practitioners. The instrument consists of two blades in lateral apposition, united at their extremities, and again about five inches lower down, being ununited in the interval. The form is that of an uterine sound. A projection two and a-half inches from the point indicates the length of the uterine cavity, and the finger can ascertain by its position how far the instrument has passed into the womb. The mechanism is borrowed from Mr. Henry Thompson's dilator for the male urethra. A small cross-bar, forming a lever, lies between the ununited portion of the blades at A, and this is made to assume a position more or less horizontal by means of a wire passed through the stem of the instrument, which is worked up and down by means of a screw (C) attached to the handle. An index at B shows how far the blades are separated in the progress of dilatation, and a ring is fixed to the under side for the insertion of the fourth or fifth finger of the left hand, to steady the instrument, while the finger and thumb of the same hand are employed in turning the screw. In this way the right hand is free to guide the point of the sound in the necessary direction, and to keep it *in situ* during the expansion.

The advantages possessed by this form of dilator are,—that it can be fashioned very small for extreme instances of contraction, where it would be very difficult to pass in a sponge-tent; and, although the flexibility of the uterine sound is not consistent with its mechanism, yet it can be made of any curve required. The dilatation produced is lateral, gradual, and progressive, without necessarily being so forcible as to rupture tissue, and there is little tendency in the instrument during the separation of the blades to work itself downwards and withdraw from the os uteri.

Further, the whole cervical canal, including the os uteri internum, can be widened by this instrument, but its greatest power of expansion corresponds to the os uteri externum, where dilatation is most required.

The instruments previously in use have lacked some of the advantages mentioned, and have thus been less useful. Among others, I possess a dilator employed by the late Dr. Edward Rigby, constructed like a pair of polypus forceps, with the blades terminating in a long and slender duckbill, for insertion into the os uteri. The instrument could not be passed into an os uteri much contracted; moreover, in any case I have attempted to use it, I have not succeeded in penetrating within the os uteri beyond half an inch, and it has slipped out of the orifice directly separation of the blades was attempted. An ingenious instrument was exhibited by M. Mathieu, of Paris, in the late Industrial Exhibition. This, also, I have employed; but it has the disadvantage of dilating in an antero-posterior direction, and in practice tends to withdraw from the canal of the cervix instead of stretching the contracted parts. This last objection also applies to those exhibited by Charriére. I do not propose here to discuss the propriety of dilating the os uteri, or to point out the instances in which dilatation should be attempted. I think it is generally admitted that cases occasionally occur in which this treatment may be pursued with advantage if it can be carried out effectually; and my own experience leads me to believe that gradual dilatation is more likely to be followed by permanent widening of the cervical canal than the more forcible and rapid distention produced by a sponge-tent.

Lastly, I may be permitted to remark that the relief experienced in some cases of dysmenorrhœa after dilatation of the cervical canal is not always apparently in proportion to the amount of previous contraction as determined by the easy or difficult introduction of the uterine sound in the menstrual interval. There are, I believe, patients to be relieved by dilatation who have no marked contraction of the orifice, but who suffer pain because the menstrual fluid is secreted more rapidly in the uterine cavity than it can escape through a cervical canal of ordinary calibre, or else, it may be, have the aperture narrowed for the time being by spasmodic contraction of the orifice such as takes place in spasmodic stricture of the urethra in the male.

I have been informed at Mr. Coxeter's that, some time ago, another practitioner had ordered Mr. Thompson's urethra dilator, without the modifications I have suggested, but with the view of employing it for dilatation of the os uteri. I have not been able to ascertain this gentleman's name; but as this short paper is simply to notice a useful instrument, and not to put forth a claim to priority, it is of little consequence.—*Medical Times and Gazette, March* 5, 1864, *p.* 251.

96.—"INSTRUMENT FOR DILATING THE CERVIX UTERI."

By Robert Ellis, Esq., Obstetrical Surgeon to the Chelsea and Belgrave Dispensary.

My attention has been drawn to the communication of Dr. Priestley in your number for last week. It was with no little surprise that I recognised in the engraving my own instrument, made for me two years ago by Mr. Coxeter. I enclose you a tracing of my dilator, by which you will perceive that the instrument is identical in every respect, in form and in mechanical construction, with that described and delineated by Dr. Priestley. It is inexplicable to me how Mr. Coxeter could have suffered Dr. Priestley to fall into such a mistake, as he must have been well aware that I originated the instrument, together with many other modifications of obstetric instruments which Mr. Coxeter has very skilfully constructed for me.

At the close of his communication Dr. Priestley makes an indistinct acknowledgment of some priority of design, but alleges that the instrument then made was devoid of his modifications. This is altogether incorrect; the only respect in which Dr. Priestley's instrument differs from my original model consists in the trifling circumstance that mine is open to its point, and his is not—a matter of very insignificant importance.

I was led to the invention of this dilator from a consideration of the facility with which I found I could pass the uterine sound even in cases of great contraction of the cervical canal. It then occurred to me that if I could design a sound which could be made to dilate, I might contrive an instrument far better adapted to effect this purpose for the canal of the cervix than any yet introduced.

After some little trouble, and with the assistance of Mr. Coxeter's mechanical skill, I contrived the excellent dilator in question, and I have had it in constant employ since that time. I have made large use of my instrument, and had purposed publishing some account of it, with cases in which it was successful, but a great pressure of other duties has hitherto delayed me. It may confirm Dr. Priestley in his opinion of the value of what he concluded to be a new instrument, to be assured that it has already done good service in practice before the idea of its design occurred to him.—*Medical Times and Gazette, March* 19, 1864, *p.* 322.

97.—ON VESICO-UTERINE FISTULA.

By James R. Lane, Esq., Surgeon to St. Mary's and the Lock Hospitals, and to St. Mark's Hospital for Diseases of the Rectum.

[This case, *i.e.*, its earlier history, was published by Mr. Lane some time ago. It was in that stage rather interesting, but the sequel has turned out so singular and unexpected, that we feel it would be an omission not to republish it. A vesico-uterine fistula we need hardly say is very rare. It must not be confounded with a vesico-utero-vaginal fistula. The following is the account of the case :]

C. R., who stated her age to be forty-five, was first admitted into St. Mary's Hospital in May, 1862. Five months previously she had been delivered of her second child, at the full period, after a labour of twenty-four hours' duration. From that time till her admission she had been totally unable to retain her urine. Her clothing was in a continual state of saturation, and her labia and thighs were severely excoriated. I found it by no means easy to discover whence the escape took place, for the vesico-vaginal septum and the urethra were perfectly sound throughout; and, judging from the difficulty attending the investigation of this case, I think it not unlikely that vesico-uterine fistulæ may have been sometimes overlooked, and the incontinence attributed to some other cause, such as paralysis of the sphincter of the neck of the bladder. I found, however, on examination with a speculum, that a small quantity of fluid could be seen to flow from the os uteri into the vagina, and I discovered further, by digital examination, that about half an inch above the os uteri, which was large enough to admit the tip of the finger, there was an opening in the canal of the cervix, leading forwards towards the bladder. The case was rendered perfectly clear when I found that a sound introduced into the bladder by the urethra could, by a little management, be brought into contact with the finger in the cervix uteri.

The question then was how to remedy the evil. I found that two plans of operation had been suggested by M. Jobert. The first was to make free lateral incisions in the cervix uteri and upper end of the vagina, so as to convert the cervix into an anterior and posterior flap, by separating which the fistulous opening could be got at, and sutures applied to it. The second plan was to close up the os uteri, so as to prevent the urine from escaping, but leaving the fistulous opening uninterfered with, and depending upon it to afford an outlet through which the menstrual fluid could escape into the bladder.

The first method was, of course, physiologically preferable; but its danger was, in my opinion, considerable, as evidenced by the history of the only case in which it was practised by Jobert, while its success was exceedingly doubtful. I therefore preferred the second plan, that of closing the os uteri, and put it in practice on the 14th May, 1862. I will not dwell upon details, but will merely say that I denuded the edges of the os uteri, and brought them together by means of four silver wire sutures. The incontinence of urine was arrested from that moment, the wound healed soundly, and the patient left the hospital quite well in three weeks. She menstruated through the bladder before she left, without inconvenience or pain, and menstruation was continued regularly in the same way for three months, when I lost sight of her for a time. Towards the end of the year, however, she applied to me, telling me that since August or September she had ceased to menstruate, that she had experienced various uncomfortable sensations in the uterine region, and had been increasing in size. On investigation, I found that there was certainly a tumour in the hypogastric region, which appeared to be an enlarged uterus; but on examination per vaginam, the os uteri appeared to be as firmly closed as when she left the hospital in June.

These facts seemed to admit of but one conclusion—viz., that the fistulous opening had spontaneously closed, or had become in some way blocked up; and that the menstrual fluid, being unable to escape, had accumulated within the uterus, and was the cause of the enlargement. Pregnancy, with the os uteri firmly closed, was a contingency which never presented itself to my imagination, nor I believe to that of any of those who saw the patient.

The appropriate treatment, therefore, appeared to be re-open the os uteri to allow the accumulated matters to escape; after which, supposing the fistula to be really closed, she would be restored to her original and normal condition.

She was re-admitted into the hospital early in January, 1863. On the 10th of January I attempted to divide the uniting substance with a small knife guided by my finger; but I found it impossible to do this owing to the mobility of the uterus, and the firmness of the uniting medium. I therefore resorted to a trocar and canula, a speculum having been introduced to bring the os uteri into view; but before the trocar would penetrate I found it necessary to hold the os uteri steady with a vulsellum, and to use an unexpected degree of force. Only two or three drops of blood escaped through the canula; but I satisfied myself that I had really opened into the uterus by passing a small bougie, which readily penetrated to the depth of about three inches. The next day I was informed that a considerable quantity of watery fluid had escaped during the afternoon after the puncture had been made, and that she had complained of pain. The discharge of water, however, had soon ceased, and had not been repeated. On the following day I learned, to my extreme surprise, that she had been taken ill during the night, and that a fœtus of about four months' date had made its appearance. The watery discharge which followed the puncture was, therefore, doubtless, the liquor amnii, and was thus clearly accounted for. The patient recovered rapidly from her miscarriage; but the cicatrix formed by my operation was, of course, completely broken down by the passage of the fœtus, and the os uteri was permanently reopened. The urine escaped through it precisely as it did before my first operation. She left the hospital for a time on the 10th of February, but shortly afterwards applied to me with an urgent request to be readmitted, in order that the original operation might be repeated. This I did on the 14th March in the same way as before, and with the same result. Nothing untoward occurred; the wound healed firmly, and the incontinence of urine ceased. She menstruated through the bladder about a week after the operation as on the former occasion, and left the hospital in about three weeks. I saw her at intervals for a considerable time. The last occasion was in September, since which I have lost sight of her; but I know she would have immediately come to me had anything unusual occurred. When I saw her in September, which was six months after the operation, she had menstruated regularly through the bladder without any difficulty. No trace of menstrual secretion had been discharged excepting through the bladder, nor had any escape of urine occurred through the os uteri; neither was there anything to indicate a repetition of the mysterious pregnancy which had followed the first operation.

The question which next suggests itself is, how conception could in this instance have taken place. Either the seminal secretion must have passed through the urethra and bladder into the uterus—an explanation which I am very unwilling to adopt,—or there could not have been an absolutely complete closure of the os uteri. If, however, there were really an aperture of this kind left, it must have been exceedingly minute; for it was quite undiscernible by repeated examination, and no appreciable amount of urine escaped through it. Indeed, the union appeared to be singularly firm, as evidenced by the difficulty I found in penetrating through it. Had the union not been so firm, I should have suggested the possibility of a portion of it having been broken down during sexual intercourse, and of an inlet for the seminal fluid having been thus provided. A small opening so caused might have closed again spontaneously after the mischief, if I may so call it, had been done. On the whole, however, I am inclined to think that a capillary channel may have existed along the track of the wire sutures, one of which had disappeared and escaped detection when the others were removed, and was not taken away till a considerable time afterwards. Should this latter supposition be correct, the fact is a very curious one, as showing how minute a communication may suffice for impregnation; and it is especially interesting in these days, when enlargement of the natural dimensions of the canal of the cervix uteri by dilatation or incision is recommended by high authorities, and frequently practised, for the cure of sterility. On this point M. Nélaton has made some humorous remarks, contrasting the size of the spermatic filaments with that of the passage they are intended to traverse, and suggesting that an enlargement of that passage, instead of facilitating their transit, might seriously embarrass them by causing them to loose their way.

It may, perhaps, be a question whether I did right to repeat the operation which I had performed in the first instance, since it had failed in permanently effecting its object, and had not prevented impregnation. In my judgment, it was the best thing that could be done—first, because, though I cannot satisfactorily explain how impregnation had been effected, I believe it to have been an entirely exceptional occurrence, and not likely to recur; and, secondly, because I preferred an operation which was attended with only trifling pain or inconvenience, and was at the same time safe and tolerably certain, to the alternative of some other proceeding, such as that first practised by Jobert, which would certainly have put the life of the patient into considerable immediate danger, and the success of which in attaining the object desired was extremely problematical.

It has now been fully established, by repeated observation, that menstruation may take place through the bladder without any detriment or inconvenience. The periodical function of the uterus is not interrupted on the one hand, nor, on the other, does the bladder in any way resent the passage through it of the menstrual secretion. At the menstrual periods the urine becomes bright red, and remains so for four or five days; and were it not for this alteration of colour the patient would often be unaware of what was taking place. The cases in which this has been most frequently tested are cases of vesico-utero-vaginal fistulæ. Here there is often extensive destruction of the upper part of the vagina and of the anterior portion of the os uteri, in consequence of which a large gaping aperture is left, the edges of which cannot be approximated by any degree of traction. These cases are often remediable in no other way than by turning the os uteri forwards into the gap, and fixing it by stitching the anterior or vaginal border of the fistula to the posterior lip of the os uteri, thus making use of the latter to fill up the deficiency, and causing it for the future to form part of the wall of the bladder, the vagina being converted into a blind cul-de-sac. This has now been repeatedly done, and with the most satisfactory results. No case of subsequent pregnancy has been recorded in *these* cases, which is an additional reason for rejecting the hypothesis that the semen might, in my patient, have passed through the bladder into the uterus. But I should mention that Jobert alludes to a report that pregnancy had taken place in one instance where he had closed the os uteri for a vesico-uterine fistula in the same way that I did in the case I have related. Nothing authentic, however, is stated with respect to it.

The ingenuity which has been bestowed of late years in relieving these distressing and difficult cases is, in my opinion, worthy of the highest commendation, and I think both the profession and the public are largely indebted to the originators and perfectors of the improved operative methods now in use, amongst whom the name of the introducer of metallic sutures, Dr. Marion Sims, late of New York, but now practising in Paris, must always be pre-eminently deserving of mention.

These operations, however, by which the os uteri is turned into the bladder, and equally those by which the os uteri is closed up, have been more than once made the subject of unfavourable criticism, on the ground that we are not justified in thus rendering a woman permanently sterile. The question may admit of a difference of opinion, according to the point of view from which it is regarded, and I grant that if relief could be afforded in any other way equally effectual, and not attended with serious danger, they would be justly worthy of condemnation; but at the same time I for one have no sympathy with the sentiment which would condemn an unfortunate woman to remain in a condition which renders her loathsome and disgusting to herself, to her husband, and to all around her, merely that she may not be debarred from the chance of performing her share in increasing the population. The room which she inhabits, it must be remembered, will smell like a street urinal, and the bed which her husband is supposed to share with her will be in a like uninviting condition. These surroundings, I fear, will not much encourage the procreation of children, but will be much more likely to breed domestic disagreement than anything else; at all events, to my mind, the chance of increase obtained by allowing such patients to go unrelieved is much too dearly brought at the price of the constant misery which it compels them to suffer.—*Lancet, Feb.* 20, 1864, *p.* 207.

98.—ON VESICO-VAGINAL FISTULA.

By George Pollock, Esq., Surgeon to St. George's Hospital.

[Before undertaking an operation for vesico-vaginal fistula it must be considered whether the state of health of the patient is good, so as to ensure the best prospect of healing—if this is not the case it may be better to defer the operation. The catamenial period must have passed over a day or two previous to the time the operation is performed; the mucous membrane of the vagina must be in a healthy condition, and the bowels must be freely acted on the night before the operation, and the rectum washed out with tepid water an hour or two before.]

As regards the operation itself the following observations may be useful:—The most simple and efficient speculum to expose the fistula is Bozeman's; with it the rectal surface of the vagina can be most efficiently depressed, and ample space secured in the cavity of the vagina for the necessary manipulation of the instruments and introduction of sutures. The position of that for the operation of lithotomy is most convenient when chloroform is administered; but if the patient does not choose to be placed under its influence, she had better be made to rest on the elbows and knees; the fistula is most readily reached in this posture. In pairing the edges of the fistula, it is most essential that the mucous membrane of the vagina should alone be removed to the extreme margin of the opening, and that the walls of the bladder be left untouched by the knife; so that, although the opening in the vaginal wall is enlarged when the edges are pared, the opening in the vesical wall is no larger than previous to the operation. The sutures should be of metal, passed on needles perforated through the shaft, so that the needle, with the suture lying partly hid in it, is first passed, and the suture subsequently pushed on through it. The texture of the suture may be silver, or iron wire silvered, or iron wire alone. The second is preferable, being cheaper than silver, and not liable to rust like the last. When the sutures are secured by a double twist, the ends should be cut off quite close to this point. The vagina should be wiped out quite dry, so that all clots and urine be removed from the cavity, and nothing left to decompose or cause irritation of the parts; and on no account should any dressing be applied to the surface of the wound. The vagina should be left free and empty.

After the operation is complete, a male gum catheter should be introduced; to the catheter a short india-rubber tube may be attached with a small flat india-rubber bottle at the further end; or a long india-rubber tube may be attached, so that the free extremity may reach over the side of the bed into some vessel there placed to catch the escaping urine. If the long tube be used, it should be tied to the bedside, so that it shall not drag on the catheter or pull it out of the urethra. The patient should lie on the side; but occasionally this is so irksome that she must be turned, so as to lie on her back. The catheter should be daily removed, washed, and replaced; a new one to be substituted every third or fourth day. Opium should be given daily in sufficient doses to keep the bowels quiet for ten days; after that an enema may be given to empty the rectum. After the action of the bowels the use of the catheter may be discontinued. The diet must be simple and rather spare until the bowels have been allowed to act. The wound should not be disturbed or examined for a fortnight; for it is well proved that the presence of metallic sutures produces no irritation if allowed to remain a month or more in the vagina after an operation for vesico-vaginal fistula.—*Lancet, Feb.* 6. 1864, *p.* 154.

99.—ON THE SURGICAL TREATMENT OF FIBROUS TUMOURS OF THE UTERUS.

By I. Baker Brown, Esq., Senior Surgeon to the London Surgical Home.

[This paper was read before the Obstetrical Society of London. On two previous occasions Mr. Brown has read papers on the same subject founded on seven cases treated by him. One of these will be found in the *Retrospect* for Jan. to June, 1861, p. 318.]

The object of the present paper was to confirm the practice previously advocated, by fourteen more cases, and at the same time to show that in most cases a very modified surgical treatment was sufficient; for whereas Mr. Brown had hitherto divided his operation into two parts—viz., first, preliminary incision of the os and cervix, and, secondly, gouging or breaking up the tumour,—he now finds that the first step will always arrest the hemorrhage and the development of the tumour. In some cases the tumour decreases, and when small it will entirely disappear, more especially if of recent origin; and even when gouging is required, a much slighter operation is sufficient. Mr. Brown therefore now never uses "Harper's instrument," but only a pair of long-handled, blunt-pointed, curved scissors. The author entered minutely into the mode of operating, and laid great stress on carefully and thoroughly plugging the incisions and whole vagina with oiled lint after the operation, as on this point depended the chances of hemorrhage, and exposure to air, and consequent hysteritis, and even peritonitis. Mr. Brown then read fourteen cases, occurring in the London Surgical Home, illustrative of his remarks. Of these fourteen, ten were cured of the hemorrhage by the incision of the os and cervix uteri alone, and one was relieved; in two only was it necessary to perform the second operation, both resulting in cure of the tumour; in six cases the tumour had either entirely disappeared or materially decreased after incision alone. Of the three deaths, one had occurred from peritonitis, resulting from exposure to cold and the restlessness of the patient; one from organic disease independent of the operation; and one from pyæmia. Out of between twenty and thirty cases occurring in his private practice, the author stated that he had had one death, in a patient whose case was complicated with hematocele, and that in his public practice he had had as many more as were now given, with no more deaths.

The following practical conclusions were drawn:—

1. That the fact of the curability of these tumours is materially confirmed by these cases.
2. That it is not necessary in many cases to do more than incise the os and cervix, thereby much lessening the danger of the operation.
3. That the hemorrhage is almost invariably arrested by the incision of the os and cervix.
4. That the cure of these fibrous or fibroid tumours by surgical means, without the danger of enucleation, is now firmly established, as proved by Dr. M'Clintock, Dr. Routh, Dr. Dawson of Newcastle-on-Tyne, as well as by himself (Mr. Brown).

Dr. Gervis wished to know from Mr. Brown whether the incisions he made in the sides of the cervix uteri were ever apt to reunite; whether, if they did, the hemorrhage recurred; and whether, if they did not, the incised condition of the cervix, in a patient who subsequently became pregnant, in any way interfered with normal parturition.

Dr. Routh considered the paper as evidence of the advance of medical science with respect to a disease many cases of which had been deemed incurable. He alluded to the process of enucleation recommended by Dr. Atlee, which he (Dr. Routh) regarded as too hazardous. It was known that many cases had ended fatally under the treatment, and he regretted they had not been published. Mr. Brown, however, had acted more nobly in publishing both his successful and unsucessful cases. Dr. Routh considered that fibroid tumours required different treatment according to whether they were in or out of the cavity of the pelvis, whether the heart and lungs were press upon, or micturition impeded, or whether in either case exces sive floodings occurred. In any case the tumour should not b meddled with unless vital functions were interfered with. Mr. Brown's operation could not be used in supra-pelvic cases because of the uncertainty of position and risk of wounding the peritoneum. In such cases he considered gastrotomy the best treatment, removing both ovaries also. In the pelvic fibroids, he agreed with the author that the tumours after bisectio diminished, and he considered this an established point in practice. The opening of the cervix he believed not only checked the hemorrhage, but enabled the tumour to be mor easily got at; it was, under the restrictions laid down by Mr. Brown, quite sate.

Dr. Greenhalgh considered the question of fibroid or fibro tumours one of great importance, especially where the hemor-rhage endangered the life of the patient. In the great majority of cases he had found the best results from the use of bichloride of mercury or bromide of potassium. Still there was a certain limited class of cases in which medicines were of little avail, and it was in such that the division of the cervix uteri wa attended with benefit. He had seen a few cases, and in these, although the hemorrhage was considerably lessened, yet the size of the uterus was not much reduced. In those cases where pressure on surrounding structures had produced injurious effects, he had seen good results from pushing up the tumour above the brim.

Mr. Brown, in reply to Dr. Gervis, stated that care was taken to prevent the incisions in the os and cervix from uniting. The oiled lint, in the first instance, and the injections afterwards, generally prevented union; but if any bands of lymph were thrown out, he always broke them down with the finger. He had never seen a case in which the patient had been confined after the operation, but it was only reasonable to infer that parturition would be accelerated after these incisions; for in the case of a primiparous woman, where the os is torn, the subsequent labours are always exceedingly quick. As to the *rationale* of the operation, Mr. Brown had avoided the question for fear of giving too wide a field for discussion. He thought the action was twofold. The tumours were of very low vitality, and the slightest interference with them stopped their growth. Thus, in the first place, the initiatory blooodvessels were cut through by incising the os and cervix; and secondly, the tumour was grasped more firmly by the contraction of the uterus, which always takes place after the os is incised; and thus the tumour was confined, could not grow, and often died. He was obliged to Dr. Routh for alluding to the authenticity of his

(Mr. Brown's) cases, as some doubt had on more than one occasion been thrown on the subject. He could only repeat that his practice at the London Surgical Home was open to every member of the profession. He had only given cases where severe hemorrhage was present as the most urgent symptom, and in almost all the cases which came to him the patients had been previously treated without success.—*Lancet, March 26, 1864, p.* 352.

100.—REMOVAL OF A FIBROUS TUMOUR FROM THE UTERUS.

By Dr. Michael T. Sadler, Barnsley, Yorkshire.

[The patient was about forty years of age, and had had no children. Uterine symptoms existed for about a year and a-half before a vaginal examination was made by Dr. Sadler.]

I diagnosed a fibrous tumour or polypus within the cavity of the uterus, as yet inaccessible. Occasional doses of ergot were given at intervals, with the hope of dilating the os, but for some time without much effect. The appetite, which had hitherto been good, began to fail, whilst the pain and discharge increased, frequently being so severe as to cause fainting. The patient's strength and spirits began to fail seriously, and I feared that it would become necessary to dilate the os uteri by sponge-tents, or other means.

On July 4, however, when Mr. Teale, of Leeds, saw the case with me, the os began, for the first time, to show signs of yielding to the internal pressure, and continuance of the same plan of treatment was advised.

On July 12 the pain was unusually violent, and, on examination, I found the os distinctly dilating, and could reach the tumour within it. On the 15th, the pains continuing very severe, the tumour began to descend into the vagina, and by 8 a.m. on the 16th, it had got so low as to press on the urethra and prevent micturition. As it now seemed fairly within reach, and the patient was suffering extremely, I determined to attempt to remove it. I accordingly emptied the bladder, put the patient under the influence of chloroform, and, finding no kind of forceps of any use, on account of the yielding nature of the tumour, and the small proportion of it which was as yet extruded from the uterus, I passed my hand (not without considerable difficulty, the patient never having had a child) alongside of the tumour into the uterus until I reached its base; then with my fingers separated it from its attachments—enucleated it, in fact, as there was no pedicle, and the rounded base of the tumour seemed to be imbedded in the substance of the uterus. This being done, the whole tumour came away without much more difficulty.

There was no hemorrhage of any consequence; but the patient was rather slow in recovering from the effects of the chloroform, and about twenty minutes after she had ceased to inhale it fell into a rather alarming state of syncope, in which respiration was with some difficulty maintained. By degrees, however, this passed off, but she long continued very sick and faint. From this time she gradually, though slowly recovered, regaining flesh much more rapidly than strength. She has just returned from a visit to Scarborough, looks almost as well as ever, but complains of slight pains in her back and legs. Menstruation is re-established, and is now attended with less pain than she has had for some years.

The tumour was egg-shaped, the smaller end having passed first through the os uteri. It was about ten inches long, and six or seven inches wide at the broadest part, and it weighed one pound two ounces avoirdupois. On section, it displayed an areolated fibrous structure, with numerous large vessels. There is a figure in Cruveilhier's "Anatomie Pathologique," 13th livraison, planche 6, fig. 2, which gives an excellent idea of its appearance. The microscope showed nothing but fibrous tissue without any cells.—*Medical Times and Gazette, Jan.* 23, 1864, *p.* 88.

101.—THE SOURCE OF HEMORRHAGE IN A CASE OF FIBROUS TUMOUR OF THE UTERUS.

By Dr. J. Matthews Duncan, F.R.S.E., Edinburgh.

[Dr. Duncan was led to notice the source of hemorrhage in this case by observing a small clot whose discoloured end, not more than half an inch in diameter, projected on the healthy, and to the naked eye, at first sight, apparently smooth and entire surface of the mucous membrane of the body of the uterus. The patient was very anæmic when first seen by Dr. Duncan. She died some little time after this, but her death was not preceded by any very copious hemorrhage.]

The uterus occupied the central region of the hypogastrium. Its upper margin was nearly on a level with the umbilicus. Its os was high in the pelvis. The cervix was hypertrophied, and measured by the extent of the arbor vitæ was elongated to a dimension of an inch and a half. A globular fibrous tumour, fully three inches in diameter, occupied the fundus uteri. It was moderately hard. Venous sinuses that would admit a small crowquill were seen permeating it in various directions. It projected downwards moderately into the deformed and expanded uterine cavity. It was everywhere surrounded by a covering of the proper tissue of the uterus, the layer between the tumour and peritoneum being more than twice as thick as that intervening between it and the mucous lining of the uterine cavity. The thickness of the latter layer was an eighth of an inch. In this layer of uterine tissue, especially in the parts adjacent to the tumour, there was developed a reticulation of enormous uterine sinuses, such as are observed in the same part in pregnancy. These were largest above and below the tumour, and contracted in dimensions as they approached the side of the uterus, to emerge from it between the serous layers of the broad ligament. The uterine cavity was deformed and extended by the projection into it from above of the fibrous tumour. It contained a brownish mucus. Its mucous surface was pale, smooth. and healthy, and to a casual observer presented nothing but a little clot about the centre of the part projected by the underlying tumour. On inspecting this clot with a glass it was found to be protruded through a small round opening which it did not nearly fill. The opening was one-twelfth of an inch in diameter, and resembled somewhat the opening of a venous sinus exposed by separation of the placenta. A probe could be easily passed through it into the uterine sinuses already mentioned. The clot was of about two lines in length, and projected from an empty flat uterine sinus.

The source of the bleeding in cases of polypus and of menorrhagia, or metrorrhagia of all kinds, is variously described according to the theoretical views of the author accounting for it. In this case it would be equally unsatisfactory and unphilosophical to seek in such theories an explanation of the hemorrhage. An evident and sufficient source is found in the large opening of a uterine sinus upon the surface of the uterine cavity. At first sight this source of hemorrhage is more formidable than it really is, for the sinuses, though enormous potentially, have in ordinary circumstances very little real capacity, on account of their flat walls being almost if not altogether in proximity. It was only during the monthly congestion of the genital organs that blood found its way along the open sinus, and oozed out into the uterine cavity. At other times the sinuses were empty; or, if only nearly empty, oozing from them might be arrested by a little blood-plug in the orifice. Whether such an obstructing blood clot were present or not, increased impetus of the blood from any cause, or congestion induced by the erect position, or by one or more of many other causes, might re-establish the oozing and the metrorrhagia or menorrhagia.

On this case many important discussions might be founded; I shall only now suggest that the projection of the tumour into the uterine cavity may have led to the opening being formed where it was found, and that similar relations in other cases may explain the tendency to hemorrhage when the tumour does so project or become polypoid, and *vice versa*. Further, the case appears to me to indicate the value of those hæmostatic measures which diminish the impetus of the blood, or moderate congestion in the pelvic viscera. Again, the source of bleeding, being in flat easily compressed venous vessels, leads to great value being placed on means that may restrain the hemorrhage by compression, however this may be applied, whether from above, or by plugging from below, or by stimulation of uterine contraction by oxytocic medicines. The good results, if any, of incising the projecting parts of such tumours may also be explained by the closure of some of the sinuses produced by the healing up of such incisions.

In the case just related, the hemorrhages and the anæmia were much greater than are observed to result from losses in consequence of lesions of the papillary vascular loops in abrasions or ulcerations of the cervix, or from simple menorrhagia flowing from the larger venous vessels of the hypertrophied mucous membrane of the body of the uterus; and the source of the bleeding had a physical preponderance quite corresponding to these circumstances.—*Edinburgh Medical Journal, Jan.* 1864, *p.* 627.

102.—ON PROLAPSUS OF THE UTERUS, BLADDER, AND RECTUM.

By I. Baker Brown, Esq., F.R.C.S.

[Mr. Brown divides ruptured perineum into cases in which a slight rent only has occurred, say of half an inch, those in which the laceration has extended to the anus, and those in which the sphincter has been torn through; this last may be called complete rupture.]

The effects of rupture of the perineum differ according to the degree of lesion. If there be partial rupture, a more extended os vaginæ, laxity of the base of the vagina, and more or less prolapsus of the walls of the passage, are consequences. If the lesion involve the whole of the perineum, the recto-vaginal septum prolapses from want of the support afforded by the parts in their normal state; and, the resistance of the vagina being thus diminished, the uterus prolapses also. At times the posterior wall of the vagina, gradually falling down, forms a pouch, in which the fæces accumulate, and the patient is never able to evacuate the intestines without pressing upon the prolapsed part, and aiding the action of the bowel with her fingers.

This form of prolapse is known as vaginal rectocele. Or the anterior wall of the vagina may prolapse, the base of the bladder falling with it, constituting what is known as vaginal cystocele.

I will proceed to state in detail the operation for ruptured perineum. Assume the rupture to be complete. The patient is placed, with or without chloroform (better with), in the lithotomy position. The legs are held by assistants, or they are strapped together to the hands and knees. An assistant stands on each side, and holds back the parts adjacent to the rupture; not putting the fingers on the mucous membrane, but just short of it, so as to make the parts tense. Then the operator, taking a long straight knife—blade and handle both long, the latter a little roughened on the back,—and holding it in the ordinary way, places the left forefinger on the mucous membrane, and marks out exactly the incisions he is about to make. Upon the care and accuracy with which this part of the operation is performed much of the facility of operating depends. Then, laying hold of the mucous membrane with forceps, the mucous membrane of the bowel is bared by careful dissection, but not cut through. Any portions of skin, or cicatrized bands—Nature's attempts at union—which may be found in the injured parts must be removed. An operator who is ambidextrous has great advantages in performing this operation. The need of marking out the lines of incision at the first step of the operation arises from the bleeding which occurs, and which greatly obscures the parts. Unless there were some guide, the operator would not be able so readily to know the extent to which he had dissected, and he might become confused. The hemorrhage is generally considerable in these cases. Formerly it was considered necessary to stop the operation in order to tie vessels. Do nothing of the sort. The bleeding must be extremely severe to justify stopping for a moment. The best course is always to go on steadily and quickly with the operation.

In inserting the ligatures I use a much longer needle than the one commonly had recourse to. With this needle a much greater sweep can be made. The needle is armed with ordinary twine or waxed Indian hemp. Taking a firm grasp of the instrument, the point is introduced about an inch from the incisions which have been made, and then it is pushed deeply down into the tissues as nearly as possible in the direction of the centre line. Introduce the needle as closely as practicable to the mucous membrane of the the vagina, and having got the instrument in this position, dip it deep into the tissues again, and bring it out on the opposite side at a point corresponding to the point of introduction. The long needle, it will be seen, enables the operator to effect with one stitch what usually requires two stitches. The needle having been passed, the loop of the ligature is seized with forceps and the instrument withdrawn. On passing the second ligature, care must be exercised not to pass it through the mucous membrane. The needle must not be pushed so deeply, else mucous membrane will be brought against mucous membrane, and there will not be perfect union. A like course must be pursued to that which I described in my lecture on ovariotomy. The needle in the one case must be brought to the extreme edge of the cut surface, avoiding the mucous membrane; in the other, it must be directed in a similar manner, avoiding the peritoneum. If the cut surfaces are brought into exact apposition, hemorrhage will stop, and there will not arise any further trouble from it.

The next step of the operation is to slip two pieces of bougie, called quills, under the loops of the ligatures, and fasten with a double twist—the surgical knot. In this way the two raw surfaces will be brought exactly together. The ligature nearest the anus should be tied first. Too great care cannot be taken to secure the accurate apposition of the parts. The second ligature having been tied, the operation is completed by two or three ordinary interrupted wire sutures, deeply dipped into each lip of the wound.

What I have now described applies to the primary operation—the operation for the ruptured perineum—in ordinary prolapsus of the bladder, or cystocele; prolapsus of the rectum, or rectocele; and prolapsus uteri. But if the sphincter ani be torn, so that half of it is lost, a different proceeding is necessary. The body of the sphincter is double, and attached to the coccyx by a tendinous insertion. The muscle embraces the anus. The anatomical structure of the sphincter renders necessary the step I am about to mention. Before bringing the cut edges together I divide the superficial fibres of the muscles on each side. If this be not done, the contraction of the sphincter, easily excited, will strain injuriously upon the apposed parts, and a recto-vaginal fistula may be the consequence.

In the first operation I performed for complete rupture of the perineum I did not recognise the necessity of dividing the sphincter on both sides, but divided it on one side only. It happened that I had not the aid of skilled nurses in this case, and I was so interested in its progress that I nursed the patient myself, sitting up with her eleven successive nights. I was thus enabled to watch very narrowly the progress of the case, and I found, on sponging the reunited parts, that while the half of the sphincter ani which had been divided did not retract, the undivided half retracted strongly when touched. After noticing this occurrence for four or five days, on placing a finger into the vagina, I felt that the reunited surfaces were giving way. On discovering this I immediately divided the untouched half of the sphincter, and so prevented further traction on the united surfaces. I happened to mention the subject to Sir B. Brodie, remarking that the operation would probably never succeed unless the sphincter were divided; upon which he said, "You must recollect that the sphincter is double." I at once saw the truth of this statement. I had not before rightly apprehended the lesson to be derived from the operation just related. From that time I have always divided both bellies of the muscle. The result is that the parts are perfectly passive while the patient is under treatment. Let me caution you not to examine the reunited parts for at least ten days after the operation. The effects may be most mischievous. I have known the introduction of the finger into the vagina to prevent complete union. Having brought the parts together, let them alone; do not be too anxious to see the result; have patience, and faith in your own work.

In the operation for ruptured perineum, as in all operations of a plastic nature, before attempting to operate great care should be exercised in selecting the assistants and in defining the precise work each has to do. If this is not done the operation will not proceed satisfactorily. The operation used to occupy a long time. I was frequently an hour, and I recently heard of a case in a London hospital that occupied two hours. I seldom now require more than a quarter of an hour or twenty minutes, because I am assisted by gentlemen who know every step to be taken; so that I have not simply one pair of hands. but three or four working in unison, and with the regularity of clockwork.

No less than 30 cases of complete rupture of the perineum have been treated in the Home in the last five years. In the same period also 33 incomplete cases, with more or less prolapsus of the uterus, were treated, and in addition six cases of vaginal rectocele. Altogether 69 cases of the lesions under consideration have, within the period named, been treated in the Home. Of these cases no less than 61 have been absolutely cured. I may add that my experience in these cases, at St. Mary's Hospital, at the Home, and in private practice, extends to upwards of 500 instances. I might multiply cases treated in the Home, and illustrating the happy results following the operation for ruptured perineum and prolapsus which I have described; but I prefer to direct attention to certain points on which the successful treatment of the lesion largely depends.

The questioned is often asked, Will a woman who has once had the perineum restored by operation again bear children without a re-rupture? I answer that, with few exceptions, she will. In the very first case I treated in practice—the case which I detailed a few moments ago, and which I sat up with eleven nights—the patient was delivered by myself, three years afterwards, in the presence of Sir Charles Locock; and although the child weighed 12½ lbs., the delivery was effected with merely a slight lateral tear. The perineum, indeed, was not injured, and the patient never lost the use of the sphincter, which she had not had for fifteen years before the operation.

It is not at all necessary that the operation for ruptured perineum should be deferred, as recommended by some gentlemen, until after childbearing, from fear of a second rupture. Indeed, it is to be borne in mind that, if a second rupture does occur, the torn parts can always be brought at once together, and successfully united, by the ordinary quill or silver wire suture. The propriety of operating immediately after the lesion has, however, been questioned. I believe, and strenuously urge, that the operation should be performed while the parts are raw. At that time they will unite in two or three days. The objection urged to this course on account of the local discharge is futile, for if the edges of the ruptured part are properly brought together they can suffer no injury from that source. At all events, in my own experience, and in the experience of my friend Mr. Harper, who has several times gone for me to urgent cases around London, where the operation has been performed immediately, in no one instance have we seen any evil consequences.

Another question which arises for consideration is this, Can a woman go on suckling with so severe an operation? Will not the secretion of milk be interfered with? My answer, founded on the experience of several cases, is, that the operation does not at all interfere with the milk after the first six hours. Chloroform seems to retard the secretion for a few hours, but the breasts recover their functions very rapidly.

It is necessary that I should now speak of the treatment to be pursued after the operation for ruptured perineum, and the different prolapses arising from this lesion. The first step to be taken is to lock up the bowels by opium, given in grain doses at intervals of six hours. The next step is to support the patient by a generous diet of wine, beef tea, and animal nourishment. Often a bottle or two bottles of wine may be required daily for the first eight or ten days, and even two or three weeks. Have no fear of the bowels becoming loaded and the patient feverish under such a diet. If never affects a patient otherwise than beneficially. The operation has frequently failed from opium and a generous diet having been withheld. On the tenth day it is well to clear the bowels by giving first a dose of castor oil, and subsequently administering frequent soft-water injections, to which a little soap has been added.

The use of laxatives immediately after the operation has been advocated, and their repetition so as to cause the bowels to act every day. When there is incomplete rupture of the perineum no harm may follow this practice; but when the rupture is complete, too early and frequent action of the bowels will prevent perfect union of the newly-united surfaces in nine out of every ten cases. A caution is necessary in the administration of opium. The drug must be given with great wariness when the patient is suckling. The child should not be put to the breast for three or four hours after a dose has been taken, or it may be poisoned. I adopt this plan, and do not permit the child to be put to the breast more than three times in the twenty-four hours.

Occasionally phlebitis occurs after the operation. When this happens, hot fomentations and gentle bandaging of the leg and thigh will be the best treatment. I have not seen death ensue from simple phlebitis, but phlebitis may lead to pyæmia. In three out of four cases of phlebitis coming under my own observation, I ascertained that the patients had been in the habit of taking large quantities of opium for several years, in order to keep the bowels costive, and diminish the inconvenience arising from a torn sphincter. In a bad case of ruptured perineum a patient is never safe unless she wears a napkin; for whenever the bowels act, they act instantly. I now make it a rule to ask the patient, before operating, if she has been in the habit of taking opium. If she has, and the skin is hard and dry, and continues so in spite of warm baths and frictions, I extend the period of preparatory treatment from a week or two to one, two, or three months. The skin is not the only organ which suffers from the opium; the patient, as a rule, is predisposed to pyæmia. During the preparatory treatment the patient must, of course, abstain from opium.

Of the cases of ruptured perineum treated in the Home but one has ended fatally. Three patients have been lost after the operation for prolapse of the uterus. The cause of death in each case was pyæmia.—*Lancet, May* 14, 1864, *p.* 543.

103.—ON THE OPERATIVE AND MECHANICAL TREATMENT OF PROLAPSUS UTERI.

By Redfern Davies, Esq., Surgeon to the Children's Hospital, Birmingham.

[The object of this paper is to show that the effect of the operations now resorted to for the cure of prolapsus uteri is usually permanent, and that the parts subsequently admit of childbearing. Speaking of the case of a young woman upon whom Mr. Davies operated, and who was subsequently delivered of a child, he observes:]

The general plan of the operation—the removal of mucous membrane from the lateral and posterior surfaces of the vagina, and the insertion of deep and superficial sutures—was conducted similarly to the method adopted by Mr. Baker Brown. But in the substance composing both these sutures, and where they are attached to, I differ entirely. Instead of twine sutures, I have always used silver wire, of a sufficient size that it may not cut the tissues, and freshly annealed that it may be of such pliancy as to be easily managed. The purpose of the deep sutures is to adapt the vivified surfaces to each other, so that they may unite by firm and immediate intention. Silver wire does this as well as twine, and at the same time is superior to it in not absorbing any of the secretions—freshly poured out or not,—so becoming of itself a local irritant or fetid stringy mass permeating tissues that are endeavouring to unite, and requiring besides to be removed on the second day, lest, as Mr. Baker Brown says, "further retention produce sloughing and suppuration." The object aimed at will, I think, be more certainly attained by the employment of silver wire, since, from the harmlessness with which its presence is tolerated by the tissues, it does not require to be removed for ten days, thus allowing time for large surfaces to become united.

The utmost possible cleanliness being desirable about these parts, after making many trials of glass in various shapes, I now use on each side three glass rods of about one inch in length, having—as suggested to me by Mr. Blake—a central nick where the wire is to be attached; and, undeterred by any fear of damaging the supports of my fabric, or the fabric itself, I am able to refresh the patient each day by copiously syringing the parts. A further advantage to be derived from using *pieces* of glass rod is, that the pressure required is diffused more equably, and to where it is wanted.

The plentiful administration of opium to the patient is not so much for the purpose of securing rest, as to prevent action of the bowels; in order that there may be no disturbance, from the passage of a stool, of the parts around the pared edges whilst they are uniting. That such a disturbance—however slight it may be made to be—is certainly best to be avoided in the amount of union that will occur where the deep sutures remain but two days, can hardly be doubted; but then, if, as Mr. Baker Brown says, "the bowels are to be constipated for two or three weeks after the soft parts have united," what is the use of "completely dividing the sphincter ani on both sides."

The plan that I have adopted has been, immediately after the operation, when there are feelings of uneasiness and smarting, to administer one grain of opium, and induce sleep, so that the patient upon awaking may be refreshed, and only to repeat the opium in case of want of sleep. In one case, from first to last, there was no opium taken. On the third morning after operation I administer an enema of warm water, and to ensure the bowels being regularly opened, and that the stools may be soft and passed easily, one teaspoonful of milk of sulphur is taken by the mouth every evening. I divide the sphincter ani on both sides most completely, and subcutaneously if possible. The result of this treatment has been that the patient has passed her stools easily and almost unconsciously for a short time. The sphincter ani has regained its original power in a few weeks, while all the effects of a mild opium-poisoning are avoided. I should add that Mr. Blake after many trials of these plans, both in the lying-in hospital and in private practice, fully, I believe, coincides in their efficacy.

Mr. Baker Brown, in the last edition of his work on the "Surgical Diseases of Women," says—"Let all pessaries as mechanical supports to the uterus, whatever their form, be avoided." That those pessaries which—as he says, *all* do—produce irritation, excoriation, and leucorrhœa, are incompatible with perfect cleanliness, or which stretch and tend to keep up relaxation of the canal, are to be avoided, few will deny. But "Zwanke's pessary" causes none of these evils. It acts, so far as I can make out, by forming an artificial floor, upon which the uterus, when no longer suspended in the pelvis by the tonicity of its ligaments, may rest, being itself supported by the soft parts on the inside of the ischium, more especially by the levator ani.

In one case—not included in the before-mentioned six—the uterus shortly after operation was felt to be high up in the pelvis, and only just within reach of the tip of the finger; but upon the patient resuming her usual household duties it gradually descended, until it was found to rest upon the perineum, when, fearing a similar result to what had occurred after two operations in another institution, I applied a "Zwanke's pessary," and with the most perfect success.

In a case upon which I recently operated, not being satisfied with the amount of union that had taken place, I made use of a triangular piece of wood, fashioned according to the surface of the parts, and having a broad slit extending one third of its length down the middle, that it might embrace and press upon the perineum on each side of the labia, and suspended it by slightly elastic straps to a belt fitting round the hips. After wearing this for four months, I now find the uterus to be in its natural site, a fair perineum, and all the previous uterine symptoms gone. She states that at the end of a day's work, without this compress, she feels a "weakness," but nothing more, about the lower part of the abdomen, and that the way in which it seemed from her sensations to act was "by pressing the parts together, and to support them when they had to withstand unusual pressure, as in lifting."

A case occurred some years ago in which I attempted to *almost* occlude the vagina. The patient, aged thirty-six, the mother of nine children,—the eldest twenty-one and the youngest nine years,—had had prolapsus uteri, gradually increasing in the degree of pain and discomfort it occasioned, since the birth of her second child, so that when she came under my care at the Birmingham Workhouse Infirmary in May, 1861, she complained that life was a burden to her, and was willing to submit to anything that offered some chance of relief. Menstruation not having ceased, I was obliged to provide for its escape by leaving a strip of mucous membrane in the lower or posterior part of the vagina, whose lateral walls were denuded of their covering as high up as the meatus urinarius, and as far back as I could get—I should say about two inches. The result of the operation is that the uterus can be felt by a bougie resting upon a thick cushion—of about an inch in depth—formed from the union of the lateral walls of the vagina, through which there is a small passage allowing the menstrual flow, and that her ailments due to the prolapsus uteri have entirely ceased.—*Lancet, April* 9, 1864, *p.* 407.

104.—ON THE TREATMENT OF IRRITABLE UTERUS BY BROMIDE OF AMMONIUM.

By Dr. G. de Gorrequer Griffith, Dublin.

[After describing the local treatment adopted by him, the writer passes on to the general mode of treatment which he employs.]

Rest of mind, and of the body in the horizontal, supine, and *pronate* positions alternately; regulation of the diet, according to the condition and habit of the patient; proper daily discharge of the alvine function, by the use of no more powerful drug than castor oil; and the administration of the bromide of ammonium in doses varying from grs. x to xx. (or even more should it be requisite), repeated every fourth hour; should we, however, wish to stop the matritic discharge altogether and quickly, we may use this medicine in the following way: for the first dose we might prescribe ℈j.—℈ij., or ʒj., to be followed every hour or every two hours by grs. x. to xx. I am now alluding to a case where hemorrhage from the uterus, in the form of menorrhagia or metrorrhagia is the urgent symptom; in those instances, however, in which pain is the predominant cause of suffering I make use of the same drug in quantities varying from grs. x. to xx., or even in larger doses, given as there may seem occasion every one, two, or three hours. If the pain assume the paroxysmal character, I have no hesitation in prescribing ʒj., to be taken at the very onset of the paroxysm; and if there be a tendency to periodicity, I order ʒss. (sometimes ℈j. will suffice) to be taken about a quarter of an hour or ten minutes before the return is expected; the medicine, after this anticipatory dose, to be continued steadily in quantities of grs. x. every third or fourth hour till the pain be eased, when there may be allowed a longer interval, the quantity of the medicine ordered not being subjected to any diminution. The effect in allaying or removing pain, in checking, or altogether causing to cease, any uterine hemorrhage, is sometimes magical.

I have not yet arrived at any definite conclusions as to the manner in which the bromide behaves itself, so as to have the effect of an anodyne at one time, and of a hæmostatic at another, or at the same time of use, to combine the action of both these classes of remedies; I only know, as an incontrovertible fact, that such felicitous results will be found to obtain. If, however, I might be allowed to express an opinion, I would venture to suggest that, in the cases of pain, this drug may work its effects by acting *directly* upon the nerves of the uterus, or indirectly through the great nervous centres, with which they are connected, altering and regulating deranged action, while at the same time restoring or tending to restore the affected parts to their wonted healthy condition. As a hæmostatic, its action, I apprehend, is otherwise than that which we have just described, and may consist in the stimulating of the uterine muscular fibres to contract, and by so doing to constringe the vessels, or in the immediate influence exerted upon the vessels themselves, whereby their tone is restored, the capability of answering stimuli regained, and the power of healthily diminishing their calibre reinstated, the laxity of their walls superseded by normal vigour and tension, so that there cannot occur exudation.

There are other diseased conditions in which, with as decided advantage, we may employ the bromide of ammonium, and of which, on some future occasion, I propose to lay the facts before the profession; they are, menorrhagia, metrorrhagia, in the latter of which are comprehended all forms of uterine hemorrhages, whether from lesions of the uterus itself, from ovarian irritation, excitation, inflammation, or any of the various diseased or deranged actions peculiar to the ovaries or adjoining parts and viscera; amenorrhœa, dysmenorrhœa, renal derangement or disease, the same condition of the uterus, of the bladder, and of the urethra; in chronic bronchitis, also, I have employed this agent, to the great alleviation of the patient's distress.

The uterine and ovarian affections, for which the bromide is indicated, are many and various, and shall be hereafter enumerated; the vesical abnormal states are cystitis of the acute and chronic forms; mere irritation; stone; paralysis (which it will likewise occasion), and hematuria.

Reasoning from analogy, I should hope, though I have never administered it, that this medicine would prove powerfully efficacious in hæmoptysis, perhaps, also in hematemesis, if not indeed in *all* forms of hemorrhages; neither have I yet tried it, as a topical external application, in the arrest of external bleeding, but I see no contra-indication to, while I see every reason to adopt, its use.

I never give a smaller quantity than grs. x. for a dose, and in certain vesical or renal complaints, I sometimes combine it with kali nitratis, to determine it the more to the kidneys, or to hasten it to the bladder, whither it is still further accelerated, by insisting upon the patient taking large draughts of some bland or mucilaginous fluids, such as flax-seed, oatmeal, or barley-tea, or a strong solution of gum Arabic.

I have found this drug also very useful in obstinate cases of phosphatic urinous deposit!

May it not in post-partum hemorrhage likewise prove as powerful a styptic as in those cases to which I have made reference in the foregoing part of this paper?

In so many uterine and ovarian affections will this drug be found beneficial, as almost to justify the attaching to it of the name utero-ovarian specific!

In conclusion, I would state, that the profession is largely indebted to Dr. Gibb for the very valuable medicine which he has placed at its disposal; but, that on the part of the public, there is due to him a very much larger debt of gratitude for his having brought forward and established in use an agent so decidedly remedial in the various forms of disease.—*Medical Circular, Dec.* 30, 1863, *p.* 372.

105.—EXTRACTION OF A HAIR-PIN FROM THE URINARY BLADDER OF A FEMALE.

By G. H. Porter, Esq., Surgeon to the Meath Hospital.

Many interesting cases of removal of hair-pins from the bladder of females, have been from time to time recorded, accompanied with practical hints for the mode of relieving the the sufferers from those foreign bodies. Among the most remarkable may be mentioned those which occurred in the practice of Mr. Syme, Mr. Hilton, and Dr. Tabuteau. The following remarks will, I trust, be read with interest, as pointing out an easy and efficient method, and as illustrating a fact already known, viz., that, the urethra of the female is capable of being dilated to a very great extent, without subsequent injury to the canal.

J. N., aged twenty, unmarried, admitted into the Meath Hospital, March 24th, 1861, stated that she had, five days previously, pushed the rounded extremity of a hair-pin up the urethra, and, as it slipped from her fingers, she was unable to withdraw it again. She did not suffer much prior to her admission, merely feeling a slight smarting after passing water. On the following morning I introduced a silver female catheter into the bladder, and drew off about two and a half ounces of turbid urine. Previous to, and after emptying the viscus, I could distinctly feel the instrument strike against a hard body. I immediately withdrew the catheter, and having had her placed under the full influence of chloroform, and put in the position for lithotomy, passed in a slightly curved narrow-bladed forceps, but without effect. I then used a long probe hooked at one end, and sought to entangle it in the bent part of the pin, but in this also I was disappointed. The pin could be felt lying across the neck of the bladder, the points being directed to the right side, but I was unable by any manœuvre to dislodge it. I then decided upon dilating the canal, and on the 26th March, placed a tent of prepared sponge, about two inches in length, in the urethra, and desired the resident pupil to allow it to remain there until the first demand to pass water, and to replace it with another fresh one during the day, after each act of micturition. This plan was steadily persevered in for two days; but on the morning of the 29th March, she refused to permit the introduction of another sponge, as she was suffering great pain in the region of the bladder, had frequent desire to empty it, and tenesmus. The urine was opaque, and tinged with blood, and I ordered a warm bath, an opiate enema, and the following mixture:—Carbonate of potash, one drachm; tincture of hyoscyamus, two drachms; syrup of poppies, three drachms; and camphor tincture to eight ounces. Two table-spoonfuls to be taken every second hour.

I directed that the tent should not be re-introduced until evening, and, not even then, unless the pain had been relieved. The patient felt so comfortable towards night that the sponge was again applied, and remained five hours in its position.

30th March, I found, on examination, that I could without much difficulty gradually introduce the index finger of my left hand fairly into the bladder. By this means I was enabled to touch the hair-pin very palpably; and, having done so, tried to disengage it. In this I was successful, and brought it on so as to feel one of the points. I now guided a narrow-bladed forceps along my fingers into the bladder, and, after the lapse of a few moments, seized it. The second point was easily found; and fixing it firmly against the top of my finger in the viscus, I withdrew the hair-pin thus grasped. It was considerably coated with phosphatic deposit, although it was such a short time in the bladder, showing how very quickly any foreign substance affords a nucleus for the formation of a calculus. The girl was now placed in bed, and ordered an opiate draught, resulting in a tranquil day, when she had perfect control over the act of micturition. She left the hospital five days afterwards quite well, maintaining complete command over the action of the bladder.

Numerous excellent methods have been used for removing hair-pins from the situation indicated, and ingenious instruments have been invented for the purpose; but I consider they are not requisite, as after a gradual but free dilatation, in the manner described, sufficient access is found for the finger (by which you can easily feel what you are doing) and a pair of forceps is the only instrument, in my opinion, necessary. In Mr. Hilton's case, reported in the Lancet of February 28th, 1863, he dilated the urethra by passing in his little finger, and subsequently the forefinger, and withdrew the pin by means of a blunt hook. He also speaks of an "eyed instrument" to slip over one of the points, if it presents itself, and thus glide to the curved part. I cannot, however, conceive this manœuvre easier than seizing the prong with a forceps, hitching the second extremity on the top of the finger in the bladder, and withdrawing altogether. I am of opinion that such rapid distension of the passage is not prudent, and I consider it likely to be followed by want of control over the organ for a longer or shorter period. Mr. Syme speaks highly of a combination of dilatation and cutting for obtaining access to the bladder; and in his case produced the former effect by bougies, and the latter by a very slight incision of its neck, by means of a narrow straight bistoury. He then removed the foreign body with a hook. He acknowledges that dilatation "if carried beyond a certain degree, is apt to produce that suppurative inflammation, which in this situation so surely proves fatal." I agree entirely with this statement, and hold that the hasty enlargement (by which I understand the canal being dilated *immediately* before the extraction of any foreign substance) is far more likely to be followed by a want of power at the neck of the viscus, or by the dangerous inflammation alluded to. The late Sir B. Brodie was also in favour of *gradual* dilatation, when it is required to enlarge the female urethra; and with respect to the mode of doing so with the sponge tent, recommends that it should be "made by compressing a piece of wet sponge between two pieces of board, in a vice, or under a very heavy weight, and not that prepared by wax." Those I used were prepared in the latter manner, and I cannot imagine why he objected to it, as they were most efficient in every respect.—*Dublin Quarterly Journal, Nov.* 1863, *p.* 270.

106.—ON HYSTERIA.

By FREDERIC C. SKEY, Esq., President of the Royal College of Surgeons, and Surgeon to St. Bartholomew's Hospital.

I know no class of diseases that more thoroughly demands the recognition of surgeons than that known under the title of hysteric or local nervous disease. So thoroughly has the medical mind been imbued with the doctrine of inflammation that almost every variety of pain, every appearance of congested vessels, is stamped as the product, or the concomitant, of inflammatory action. The indispensible companionship of *pain*, *heat*, *redness*, and *swelling* is entirely ignored, and passive congestion—producing redness alone, minus pain, minus heat, and minus swelling—stands alone to claim the title due only to the sum of all. How often, in the practice of many surgeons, does not simple congestion of vessels obtain for itself, not the title merely, but the reality of treatment of true inflammation; And not less striking is the similar error as applied to *pain*. Pain is a disease *per se*. It stands alone, unaccompanied either by heat, redness, or swelling. We have local pains in various parts of the body—pains in the chest, the stomach—pains in fibrous tissues, of a rheumatic character. There is scarcely a part of the frame exempt from pain, which is an aggravated state of the sensory filaments of the nerves. How often is the term inflammation coupled with it, and depletive treatment resorted to, to the increase rather than to the reduction of the evil! Such is the disease known under the objectionable title of *hysteria*, than which the study and observation of no disease is more indispensable to the practical surgeon. A few authorities, and those among the highest in our profession, have declared that hysteria prevails more or less actively in the large majority of diseases, whether medical or surgical, especially in the upper classes of society, but also that it largely pervades the lower. What has become of the thousand cases of supposed disease of the vertebræ in young women—the "spinal affections," as they were termed, that excited so large a share of interest in the professional mind forty years ago? What has been the destination of the thousand "inclined planes" employed in the treatment of the aforesaid "spinal affections," with the train of setons, issues, and blisters, that were pressed into the service, so greatly to the advantage of the constitutional health! Which of us amongst the seniors of our profession can fail to recall in abundance cases of "spinal affection," the diagnosis of which was based on the single feature of pain caused by pressure on the spinous process of a single vertebra? We almost blush from a sense of shame as we recall to mind the occasions of our ready adhesion to this unscientific and preposterous doctrine in days gone by—a doctrine worthy of no higher or more useful purpose than that of upholding a false argument in a court of law. What are these cases? What is the supposed nature of the disease in question? Is it caries? Is it necrosis? Is it an affection of the medulla spinalis, of bone, or of fibrous tissue? Is it true or untrue that pressure of the finger on a spinous process can detect caries of the body of a vertebra? Do we ever find caries involving one vertebra only? Yet on this single feature of pain, at one point, unsupported by any collateral evidence of spinal disease, hundreds and thousands of young women have been thrown out of society, deprived of education and of many of the appliances of health, to live a factitious and artificial life for years.

There is another and scarcely less frequent variety of hysteria, which consists in a loss of the harmony of action of a set of muscles. Flexors and extensors may fail in their proper antagonism; the neck may be permanently twisted; the forearm firmly and immovably bent; the hand closed, or the leg drawn upwards. Such cases are not rare; on the contrary, they are frequent. Are they commonly recognised as affections of the general health, and as not amenable to the local treatment which is too often resorted to for their cure? Although the indication of disease points to the muscles, there can be no doubt that the seat is really referable to the nerves which supply them.

Of cases of hysteria under the form of painful joints I have had several examples of more or less duration and severity. Though occasionally obstinate, they yield to the local application of opium in extract, and of pressure by means of adhesive plaster; constitutionally, to bark, valerian, and ammonia. The spinal cases of hysteria are marked by the severity of the pain, the intensity of which characterizes no true disease of the spine with which I am acquainted. They yield to the same local remedy, to active friction by the hand, and to the same constitutional agents as are above enumerated in the examples of hysteric joints. Of muscular affections I propose to mention three examples selected from a list of many.

A young woman, aged twenty-eight, was admitted in February last. She was sent to the hospital from the country, at the instigation of the medical attendant of the family in which she filled the office of lady's-maid. Several weeks prior to her arrival she had experienced some difficulty in swallowing, for which malady medical advice was resorted to. In the course of a week or more she became unable to swallow any description of solid food, and a second medical opinion was obtained in consultation. A probang was introduced, but its progress was arrested at about four inches below the pharynx, and it was withdrawn. A second and a third attempt was unsuccessfully made to overcome the obstruction. The food was confined to liquids, which were given in small quantities at short intervals. The patient lost flesh from defective nutrition, and after a period of seven or eight weeks the disease was apparently on the increase, and she was sent to the hospital. When I saw her on her arrival she appeared cheerful, and though thin, was not otherwise unhealthy. Having learned the particulars of her case, partly from her own lips and partly by letter, I had no doubt whatever that it would prove to be a case of hysteria. It appeared obvious that it must be so, simply because it was most improbable that it could be any other disease. The symptoms of real stricture of the œsophagus do not coincide with the history of this case, and it is a far more protracted disease in its progress. Under the free employment of bark, valerian, and wine, she rapidly improved. At the expiration of a fortnight she could swallow minced animal food, and in three weeks I saw her dining off a rump-steak, which appeared in detail to pass into her stomach without any kind of difficulty. Before leaving the hospital, she expressed a wish that I would pass a probang or bougie down her throat—a wish which I declined to comply with.

A girl, aged seventeen, was admitted with what was termed "inflammation of the elbow joint." I learned that she had sustained a severe fall on the arm several months prior; that the arm became gradually bent at the elbow-joint, and was now fixed at a right angle. She had worn splints for some weeks. There was neither heat nor swelling about the elbow-joint. The joint was not fixed; and on attempting to extend the forearm, the extension, though slight, was made by jerks spasmodically; and on the remission of the extending force, the arm returned immediately to its former situation. This state of the muscles is highly characteristic of hysteria. At the expiration of ten days, during which the girl's health was much restored, under the influence of chloroform I straightened the arm without the smallest effort. The limb was placed on a straight splint, which was removed at the expiration of four days; and ten days later the girl left the hospital quite well. I subsequently learned that the fall had occurred many weeks prior to the date of the first symptoms affecting the joint.

I quote the third case from my private notes:—

A young lady, aged fourteen years, was the subject of extreme inversion of the right foot. On inquiry, I ascertained that about eight weeks prior to my interview with her she exhibited a tendency to turn the foot inwards in walking, and that the abnormal position of the foot increased from day to day until the toe of the affected foot pointed behind the opposite heel. On orthopædic surgeon was then consulted, who ordered a modification of Scarpa's boot, with all the appliances of straps, buckles, and springs. A second authority confirmed the view of the first; and for many weeks the patient was subjected to the restraint of the apparatus. Could a disease of this character, occurring almost suddenly in a young girl, otherwise not apparently unhealthy, be any other than hysteria? I ordered her wine, ammonia, and valerian. It is no exaggeration to assert than within three weeks her foot was perfectly restored to its natural situation; and what is not less worthy of remark is, that the entire period of restoration occupied less than twenty-four hours—I mean, that her recovery was almost sudden. This patient, I am informed, has subsequently had a slight return of her malady.—*Lancet, Jan.* 9, 1864, *p.* 31.

107.—ON THE PHYSIOLOGICAL ACTION OF THE UTERUS IN PARTURITION.

By Dr. ARTHUR SCOTT DONKIN, Lecturer on Medical Jurisprudence to the University of Durham, Neville Hall College, Newcastle-on-Tyne.

[The writer commences by stating that the object of the present paper is to consider the question, To what extent does the uterus contract during labour? in other words, Does the entire organ contract during a labour pain? He first reviews the various hypotheses which have been from time to time advanced by other observers.]

Wigand, first of all, advanced the hypothesis that the contractions of the uterus in parturition are essentially peristaltic, commencing in the cervix, and gradually advancing into the body and fundus of the organ, and then pursuing a retrograde course to the point where they originated.

This theory of Wigand has been adopted by Muller, Michaelis, Rigby, Churchill, Tyler Smith, Farre, and others; and the data on which it is supposed to be established consist of the following phenomena witnessed in the early stage of parturition, before rupture of the fœtal envelopes, and the discharge of the amniotic fluid. 1. So soon as a uterine contraction at this period begins to develope itself, the cervix is felt to become tense. 2. The head of the fœtus, or presenting part, suspended in the surrounding fluid, recedes from the orifice of the uterus, while the orifice itself becomes filled with a bladder-like protrusion of the amnion filled with fluid. 3. In the course of a few seconds later the fœtal head is felt to descend again, or to be forcibly protruded against the os uteri. Now, it is argued that if the uterine contractions, which expel the fœtus, had in such instances commenced in the fundus of the uterus, and travelled downwards to the cervix, the head of the fœtus, instead of receding at first from the uterine orifice, would be pushed forcibly against it.

Dr. Murphy, so far as I am aware, was the first to dispute the accuracy of Wigand's conclusions based on the phenomena just referred to. "In Wigand's explanation," say Dr. Murphy, "the influence of fluid pressure seems to be altogether forgotten. The immediate effect of contraction commencing at the fundus would be to compress the liquor amnii, which of necessity forces its way on to the mouth of the uterus. The fluid in this position reacts against the head with a power equal to that which compresses it, and therefore pushes the head up until the increasing contraction of the fundus forces it down again, so that the phenomena quoted are quite consistent with the statement that uterine contraction begins at the fundus; in fact, it could not be otherwise so long as the waters remain in the uterus. But if the contraction commenced from below, the fluid must be driven upwards, and that portion between the os uteri and the head pressed aside, at least in the first instance, so that the head might be easily felt when the pain commences, although not so afterwards."

The tension of the cervix constituting, according to Wigand and his followers, the earliest manifestation of uterine contraction, is considered, and very justly so, by Dr. Murphy, to be merely passive, and the result of the mechanical pressure of the propelled fluid against the inner surface of the lower segment of the uterus. Dr. Murphy considers uterine contraction to be peristaltic, and, in opposition to the theory of

Wigand, maintains that it commences in the fundus, and pursues a course towards the orifice of the uterus.

Braun also maintains that the contractions of the uterus do not begin in the cervix, but at the orifices of the Fallopian tubes, thence expanding equally over the entire circumference of the organ, its centripetal action being thus developed, which, as in other hollow muscular organs, is in the direction of its outlet.

Scanzoni offers the same objections to Wigand's theory as those advanced by Dr. Murphy, and urges that the simultaneous protrusion of the bag of membranes at the os uteri, and the ascent of the head at the invasion of a labour pain, are incompatible with uterine contraction beginning at the cervix. This distinguished obstetrician urges that a normal uterine contraction commences neither in the fundus nor lower segment of the uterus, but is equally spread over the entire organ.

The most recent contribution on this subject, with which I am acquainted, is that of Dr. Christie, of Aberdeen. This observer "doubts greatly if the action of the uterus in labour is of the kind that can, strictly speaking, be called peristaltic. The contraction, it is true, begins in the fundus, and then extends downwards over the body and cervix of the organ; but one point is not relaxed as the contiguous one begins to contract. From the first to the last moment of contraction, the fundus is active." "For many years past," says Dr. Christie, "I have rarely omitted to make observations in regard to the matter, and in no instance have I ever been able to detect anything like peristaltic motion."

Dr. Christie rejects Dr. Murphy's speculative interpretation of the phenomenon, on which Wigand chiefly based his theory of uterine contraction, namely, the retraction of the head of the foetus at the commencement of a labour pain, and advances the proposition that this is simply an apparent, not a real, occurrence. "It is," he remarks, "wholly due to the accumulation of fluid between it and the membranes; and the matter has been so often investigated and reinvestigated in the course of my own practice, that I feel no misgivings in insisting on that just stated as the true explanation of it."

It is obvious, then, that notwithstanding the opposite views entertained by these authorities concerning the point of origin and direction followed by a uterine contraction, they are unanimous on one point, namely, *that in each contraction the entire organ participates, the cervix included.*

The observations just quoted from the opponents of Wigand, are undoubtedly sufficient to demonstrate that his hypothesis of uterine action is untenable, and founded on a misinterpretation of certain phenomena. In the following observations I will not attempt to decide whether each parturient contraction begins simultaneously throughout the entire uterus, according to Scanzoni; or whether, according to Murphy, Braun, Christie, and others, it commences in the fundus, and then travels downwards to the cervix. This is a question irrelevant to the object of the present inquiry, which is to determine whether the *entire uterus is, or is not*, thrown into a state of contraction during a labour pain. This is a question which, as I shall afterwards attempt to show, has a most important relation to the pathology of *placenta prævia*, and is, therefore, deserving of the most careful consideration.

The following propositions will best express what I am about to advance on the subject :—

Proposition I.—During natural parturition, the seat of contraction is the fundus and body of the uterus; contraction ceases entirely in the lower segment of the uterine body.

Proposition II.—The cervix and a short zone of the body of the uterus, continuous with and adjoining it, do not contract with each labour pain, but, instead, undergo passive mechanical expansion, by which the canal of the cervix is dilated, so as to permit the escape of the fœtus and its appendages.

Physiologically, then, during parturition, a line separates the lower or cervical zone from the lateral and fundal regions above. *Below* this line there is passive mechanical expansion ; *above* it, active rhythmic contraction.

Immediately after the gravid uterus has expelled its contents, the cervix becomes passively contracted, like the so-called sphincter of the bladder, its contraction being induced by the removal of the distending force.

Now, these propositions I shall endeavour to establish by the following data :—

1. *Anatomical and Histological.*—There is no principle better established in physiology, than that an organ, in order to perform a special function, requires to be endowed with a special organization ; and we find the cervix uteri, on careful investigation, to be no exception to this rule ; for when compared with the body of the organ—(its lateral and fundal regions)—it presents in its anatomical and histological conformation differences so characteristic and unmistakable as to be considered by some almost a distinct organ. These differences are as follows :—

In the uterine body (fundus included) the middle coat, consisting chiefly of involuntary muscular fibre, united by a moderate quantity of connective tissue, is so closely united *externally* to the peritoneal coat, that they cannot be separated in many parts except after prolonged maceration ; while the mucous membrane investing its inner surface is everywhere smooth, and covered with the orifices of its utricular glands or follicles.

The middle coat of the cervix, on the other hand, is not invested with peritoneum, except only a portion of its posterior surface, and even this is separated to a considerable extent from the peritoneal investment by a mass of loose connective tissue; and a similar layer of this tissue also intervenes between the cervix anteriorly and the urinary bladder with which it is in anatomical relation. M. Jobert was the first, if I mistake not, to direct special attention to this important relation between the uterus and peritoneum. This accurate anatomist observes, that it may be established as a law that the peritoneum is intimately connected with the body of the uterus by muscular fibre, both in woman and the lower animals,—never by areolar or yellow elastic tissue; while, through the entire mammalian series, lax areolar or connective tissue is the means of union between the peritoneum and the neck of the uterus, the vagina, and large ligaments.

In the next place, the middle coat of the cervix differs remarkably from that of the uterine body. *First*, in the absence of a superficial layer of longitudinal muscular fibres; *secondly*, in the much more circular arrangement of its muscular fibres, which, according to Jobert, "constitute semicircles and decussate without mingling, the semicircular arrangement being more evident in women who have had children than in others;" *thirdly, in the great preponderance of elastic fibrous tissue over the muscular element;* this fibrous tissue consists of long detached bundles of fibres, stronger and tougher than the connective tissue which unites the constituents of the muscular and mucous coats of the body of the uterus. The middle coat of the cervix, then, *has much more of a fibrous than of a muscular character.*

The mucous membrane of the cervix differs remarkably from the smooth glandular lining membrane of the body of the uterus. To borrow the description of Dr. Farre, it is everywhere folded into *plicæ*, "which constitute a series of primary folds, from which others of a secondary order are produced. These emerge from either side of the lateral plicæ, and, crossing the furrows between them, subdivide, divide again and again, until the whole surface presents that cribriform aspect which can be just discerned by the naked eye, but cannot be accurately explained without the aid of the microscope." The depressions between the smallest or ultimate series of plicæ are occupied by numberless mucous crypts, which appear to furnish the peculiar mucous secretion of the cervical canal. But I may observe that such a complex and intricate folding of the mucous membrane of the cervix is unnecessary for the purpose of supplying merely a a secreting apparatus of a very simple description.

The chief peculiarities, then, of the cervix (which undergoes hypertrophy in all its parts, during pregnancy, without sustaining any change of contour, or becoming towards the end of that period, gradually incorporated into the cavity of the uterus, as shown by the researches of Professor Stoltz, of Strasburg, and more recently by those of Dr. Matthews Duncan) are the histological composition of its *middle coat*, in which predominates a tissue always largely employed in the animal economy, where-ever great elasticity, tension, and freedom of motion are required, and the nature of its lining membrane, or inner coat, which is folded up in a manner so complex as to give the greatest possible extent of surface in the smallest possible space, and to permit it to expand, or rather *unfold*, during childbirth to an almost incredible extent, without suffering laceration. Besides, its relation with the peritoneum and urinary bladder is such that it inflicts no fatal injury on them by the change in size and shape to which it is subjected,—first, by mechanical expansion, and afterwards passive contraction.

In the mechanism of the cervix, in short, we perceive an admirable contrivance to enable it to undergo, during parturition, a degree of dilatation unparalleled in the human organism, a dilatation so excessive that its canal, which, before labour in primiparæ, is closed, becomes, at the completion of the first stage of that process, expanded to such a degree, that it permits the escape of the head of the matured fœtus, measuring, in its shortest diameter, not less than 3½ inches, and requiring, for its passage, a circle of about 11 inches.

2. *The pathology of placenta prævia* affords important evidence in confirmation of the propositions I have advanced. In cases of this nature extended practical observation, especially the researches of Dr. Barnes, of London, have shown that the whole of that portion of the placenta (whether the entire organ or a portion of it) *abnormally implanted within the lower or cervical zone of* the uterus *is detached or peeled off during the first stage of labour, while the other portion, normally seated beyond or above the boundary of this zone, retains its attachment until after the expulsion of the fœtus.* In such instances, then, it is obvious that the condition of the uterus, during labour, *above* the line of spontaneous detachment, must be very different, or diametrically opposite, to that portion *below;* and as the condition above is that of active rhythmic contraction, the opposite condition below must be one of passive or elastic expansion from mechanical agencies. In no other way can the phenomenon just referred to be explained.

If, as I have urged, the cervical region of the uterus is mechanically stretched, during the early stage of labour, to such a degree that its outlet attains a circumference of about 11 inches, it follows that this region must undergo much alteration in shape; it must, indeed, be so shortened at the period when the fœtal head passes through it as to form a short band bordering the adjoining portion of the uterus above. That it does actually become so shortened is strikingly illustrated in those cases of *partial placenta prævia* in which only a portion of the placenta is implanted within the cervical zone, its margin reaching the os internum. This portion, of course, is peeled off during the first stage of labour, and at the completion of that stage is found protruding to a considerable extent from the orifice of the uterus, while within the orifice the rest of the placenta is normally adherent.

3. *The gradual thinning, and at last complete obliteration, of the cervix*, as labour progresses, so that at the commencement of the second stage the cavity of the uterus and the dilated vagina form one continuous canal or passage, uninterrupted by the cervix dipping like a fold into the latter organ, affords an important fact in proof of the accuracy of the views I have advanced.

4. *Practical observation* convinces me that the cervical region of the uterus does not contract during a labour pain; the rigidity of which it is then the seat is not evidence of contraction, but the effect of the distending mechanical forces to which it is subjected; these being the pressure of a fluid wedge before rupture of the fœtal envelopes, and the presenting part of the fœtus afterwards. I have made the subject one of special investigation in numerous instances, and have had the matter convincingly illustrated in those cases of tedious labour in which the anterior portion of the cervix, incompletely expanded, protrudes as a fold between the os pubis and the head of the fœtus, preventing the further descent of the latter. When this fold or projection of the cervix is gently slipped over, so as to embrace the head, it can be felt stretching or expanding under the finger during a labour pain. Were this region of the organ to contract, it would greatly impede or entirely suspend the progress of labour.

It follows, therefore, that there is a perfect analogy between the parturient uterus and the urinary bladder during micturition; the mechanical resistance of the cervical region of each organ being overcome by forces acting from above, in the one instance quickly, in the other slowly; the only difference being that the contents of the uterus, being solid and bulky, meet with a much greater degree of resistance, and require for their expulsion a series of frequently-repeated efforts, between each of which there is a period of repose, while a single contraction is sufficient to evacuate the fluid contents of the bladder.

Now, if the propositions I have advanced concerning the mechanism of parturition, as regards the uterus, be correct—and I have endeavoured to demonstrate their accuracy—we are at once furnished with a key to the establishment of a correct pathology of placenta prævia, and with an intelligible explanation of certain important phenomena inseparably connected with that form of dystocia. Thus we are at once able to perceive why the encroachment of the placenta, even to the slightest degree, within the cervical zone of the uterus is incompatible with normal labour; why any portion of the placenta thus abnormally seated is detached during that process, and its separation attended by hemorrhage of a character opposite to the accidental and postpartum varieties, in being synchronous with uterine contraction and ceasing in the intervals of relaxation; and, lastly, *why this unavoidable hemorrhage is spontaneously and permanently arrested at a certain period during the progress of labour*, as I will attempt to show by the following brief observations.

The clinical researches of Dr. Barnes have unquestionably shown that the period of spontaneous cessation of prævial hemorrhage coincides with the period of complete detachment of the placenta from its abnormal adhesion within the cervical zone, however slight or extensive that may have been. But Dr. Barnes was unfortunate in attributing the production of these phenomena to "contraction of the cervix;" because both are due to its mechanical dilatation. In cases of placenta prævia during the first stage of labour, this expansion or unfolding of the cervical zone of the uterus and detachment of the placenta from it, proceed *pari passu*, until the period arrives when the portion of the latter abnormally seated is entirely cast off. The sudden arrest of the hemorrhage at this period is not due simply to the completion of placental detachment, but *to the degree of expansion which the cervical zone has then undergone: the utero-placental vessels, previously divided on its inner surface, are by this expansion or stretching of its tissues mechanically compressed and occluded in passing through its substance.* In other words: *the remarkable change of contour—of circular stretching and shortening—which this region of the uterus has then been subjected to, and the disturbance of the natural relation of its tissues are so extreme, that blood-vessels can no longer permeate its substance as cylindrical canals pervious to the blood. This mechanical squeezing of the divided utero-placental vessels continues until the fœtus is expelled; passive contraction of the cervix then replaces the previous passive expansion, and in its turn is powerfully hæmostatic in preventing a recurrence of the hemor-*

rhage after the uterus has been emptied. This is the mechanism by which the pre-partum hemorrhage in placenta prævia, whether partial or central and complete, is spontaneously and permanently arrested.—*Edinburgh Medical Journal, Dec.* 18[illegible], *p.* 523.

108.—A NEW UTERINE PORTE CAUSTIQUE.

By Dr. Frederic D. Lente.

An efficient uterine porte caustique by which the nitrate of silver may be applied thoroughly to the whole extent of a diseased cervix, or to the interior of the body of the uterus, has long been a desideratum. Every physician who has been in the habit of treating diseases of the uterus to any considerable extent, and especially by local medication, must have experienced constant annoyance from the difficulty of making applications with any of the appliances hitherto recommended. It is probably to this cause more than to any other that the intractible nature of chronic inflammation of the cervix (cervical leucorrhœa) is due. I have found the little instrument here described by far the simplest and most efficient means of applying nitrate of silver to the cervix and body of the uterus, of all the contrivances that I have seen. It has been tried by a large number of physicians in New York, Boston, and other cities, and has given great satisfaction. I have therefore ventured to recommend it publicly to the profession. It is simply a long silver probe attached to a handle, with an olive-shaped enlargement an inch and a half from its extremity; the whole instrument should be just ten inches in length. The enlargement is for the purpose of showing when the extremity has reached the internal os, so as to prevent the caustic from touching the interior of the body of the womb when this is not advisable, if it is to be passed into the body, the enlargement will offer no resistance. It is armed with the nitrate by melting the latter in any convenient vessel; then, after having *cleaned the end thoroughly* and *heated it moderately* in a *spirit lamp*, by dipping it repeatedly in the liquid caustic, after it has cooled a little, until a sufficient coating is lodged upon it. In applying it, expose the os with the bivalve or with Sims's speculum, pass the sound to get the exact curve and direction of the cervical canal; bend the end of the probe to correspond, and then it can be passed directly to the internal os, or to the fundus, without losing any of the caustic in exploring, or unduly irritating the cervix. Just so much of the nitrate may be placed on the probe as may be thought necessary to leave in the uterus; it may be rubbed over the whole diseased surface repeatedly and firmly, and withdrawn, without any danger of detaching it from the probe, if the above directions are observed. The instrument may be had at Tiemann's and at Otto and Reynders's.—*Amer. Med. Times.*—*Dublin Medical Press, Oct.* 28, 1863, *p.* 507.

109.—ON A VARIETY OF CHRONIC PAIN IN THE BACK.

By Dr. Henry Gervis, Assistant Obstetric Physician to St. Thomas's Hospital.

[Dr. Gervis relates the following case and makes some remarks upon it which are well worth remembering.]

In April, 1860, Mrs. D., aged thirty, came under my care as an out-patient of St. Thomas's Hospital; she had a somewhat worn aspect, but did not appear otherwise unhealthy; her particular complaint was of a constant pain in the lower part of the back, which she stated that she had suffered from with but little intermission, since the year 1854, the date of her last confinement. She had been treated for it by both private and hospital physicians, and they all had spoken of it as a "weakness," connected with uterine disease; thinking it, I presume, that sympathetic "dolor dorsi" which constitutes so frequent a symptom in the history of uterine affections. She had however, never suffered from any other symptom of such disease, excepting an occasional catamenial irregularity and an occasional slight leucorrhœa, but at the time I first saw her, the catamenia were regular and she had no abnormal discharge, and yet the pain in the back was both constant and severe.

On a vaginal examination, digital and specular, the uterus was found somewhat large but without tenderness, and otherwise healthy. No disease, in fact, was detectible to which this chronic pain could in any way be ascribed. On a careful examination of the back, I found that she defined the pain as being especially connected with the left sacro-iliac synchondrosis; on percussion or pressure upon this articulation there was a marked amount of tenderness, and on taking hold of the ala of the corresponding ilium, and pressing it backwards and forwards, she experienced a considerable increase of the pain. On further questioning her, I found that the pain was least when she was lying at rest; worse when she walked, and greatly aggravated by a false step, a sudden twist, or any other unexpected movement which affected the articulation, as in slipping over the kerb, missing a stair, treading upon a loose stone, &c. Her general symptoms approximated to these of slight hectic; there was some variable febrility, some loss of flesh and appetite, and a sense of weariness, induced by the constancy of the pain. I could detect no local external fulness or thickening, but the tenderness was considerable, and limited to the synchondrosis. She was not a rheumatic subject, and though never very robust, had previously to the occurrence of this pain enjoyed fair health. I looked, therefore, upon the case as one of a chronic congestive, or subinflammatory condition of one or more of the tissues of the synchondrosis, following indeed her confinement, but unconnected with existing uterine disease. It was treated upon ordinary principles, by rest, the local application of blisters in narrow strips, of about two inches long and one broad (within a limit I defined by the pen); and as an internal medicine she took the bichloride of mercury in decoction of cinchona, three times a day, and at night some extract of henbane and Dover's powder, with an occasional aperient. She soon began to improve, and within eight weeks was completely relieved from a pain which had previously scarcely left her for six years. Her general health became also re-established. I of course should not have ventured to bring such an ordinary surgical case before the notice of this society had it not happened that by several practitioners this pain had been considered wholly dependent upon an associated uterine disease, and that since the occurrence of this case, I had seen others in which a similar affection of one or other synchondrosis constituted their most important element.

Of the post-mortem pathology of these cases I am unable to speak, as, fortunately for the patient, they rarely permit the scalpel or the microscope to aid in their elucidation.

In some cases there is a pain in the back, apparently of a rheumatic character, affecting the ligamentous and fibrous tissues about the articulation, and in some the periosteum is evidently affected. In these the iodide of potassium in combination with alkalies and other antirheumatic remedies is of especial service; but in others, as in the case detailed, the affection is certainly of the deeper-seated tissues, the cartilage and fibro-cartilage, viz., which connect the bony surfaces. In four cases which I observed the pain came on after parturition, and in two the child was said to be an unusually large one. Possibly, in these, this was the direct mechanical cause of the subsequent condition of the articulation, the undue strain upon it originating that condition of its tissues to which the chronic pain which followed was referable. The patient whose case is detailed had borne no other child since the first occurrence of the pain, but another patient, in a labour subsequent to the one from which she dated the commencement of the pain, suffered much from it during her labour and from its aggravation afterwards. I need scarcely refer to the opinion founded upon what is said to occur in quadrupeds, that the human pelvis during labour undergoes some enlargement of capacity by a slight yielding of its articulation, as I believe it has been pretty generally abandoned, but even if it were so, in those cases in which the condition under consideration appears to originate in a mechanical pressure or tension during parturition, the ordinary pressure must have been more than usual, or the capacity to yield less. But in other cases, with an equally defined affection of the synchondrosis, no such origin was assigned, but it was variously ascribed to a cold, a strain, "weakness" from some womb affection, &c. I have never met with it in the nullipara. Sometimes there is some varying increase of pain during the catamenial period, and this might easily suggest the idea that some uterine mischief was the cause of the backache; but as this variety of pain in the back may certainly coexist with a healthy uterus, the increase of pain at the monthly period is probably due to that customary amount of lumbar uneasiness which often accompanies the periodic congestions of the uterus, and which, in these cases appears to the patient to amalgamate with her more constant pain; though really, as regards its cause, distinct. I presume, then, these cases to be mild forms of an affection whose severe form would be represented by abscess, and I think that the cases which occasionally occur of subacute or chronic inflammation, and of threatened or actual suppuration of the symphysis pubis occurring generally after a severe labour from large size of the child, and whose diagnosis is unmistakable, are confirmatory of the view I would suggest of the cause of an occasional form of chronic "pain in the back."—*Obstetrical Transactions, Vol.* 5, 1864, *p.* 76.

110.—ON DISPLACEMENT OF THE BLADDER AS A CAUSE OF TEDIOUS LABOUR.

By Dr. W. H. Broadbent, Assistant Physician to the London Fever Hospital; Physician to the Western General Dispensary.

Undue distension of the bladder, prolapsus with accumulation of urine in it during labour, have long been known as sources of danger, and causes of pain and delay; and especially prolapsus with distension, as a mechanical impediment to the passage of the child's head, and as endangering the integrity of the organ, has received the consideration it demands; but I have been led to conclude that displacement and other conditions of the bladder, without involving the same serious consequences, frequently cause labour to be tedious and painful in the first stage, interfering with the dilatation of the os uteri by the substitution of false pains of a very severe character, which give much unnecessary suffering, and exhaust the strength of the patient before the expulsive stage comes on.

The conditions of the bladder which I have found thus to interfere with the first stage of labour are:

1. Complete prolapsus, with or without distension.
2. Partial prolapsus, *i.e.*, where the fundus remains between the uterus and symphysis pubis.
3. Distension while *in situ*.
4. Irritability?

They all appear to affect the progress of labour in the same way, and the attendant symptoms are similar in each, and very characteristic.

1. *Prolapsus.*—This displacement does not occur suddenly during labour, but in most cases is present before impregnation, as a result usually of repeated child-bearing. It may give rise to little or no inconvenience during pregnancy, and not uncommonly gestation goes to its full term, with no further annoyance than a frequent desire to pass water. Sometimes, however, the urine cannot be retained, and it dribbles away constantly, or the bladder is never thoroughly emptied. At any time during the later months of pregnancy the bladder may become distended, false pains of great severity may be set up, and labour may even be induced prematurely.

More commonly, however, gestation goes on to the full term, with only the slighter inconveniences first mentioned, and only when labour commences does this condition of the bladder give rise to serious trouble. The preliminary uterine contractions, generally painless, and often unnoticed, affect the prolapsed bladder; there is usually frequent desire to pass water, and if the bladder cannot empty itself the urine accumulates and distension results. But even if the bladder remains empty, the contractions of the uterus are soon accompanied with pain in the bladder, from pressure or traction, which becomes more and more severe. This pain seems to excite the uterus to more frequent contractions, and looking only at the frequency and sharpness of the pains, the labour may appear to be progressing rapidly. The pains, however, have a peculiar and distinctive character. The intervals are shorter than in the early stage of natural labour, and when inquiry is made, the pain is referred not so much to the back, as to the "bottom of the stomach," by which phrase women often mean not the hypogastric region merely, but the vagina and vulva; in this case its real seat is the bladder. The appearance and behaviour of the patient is different; so long as she chooses to remain on her feet and walk about, instead of placing the hands on the loins she prefers to lean on the back of a chair, or to sit down bending forwards with the hands or elbows on her knees, and though these attitudes are common in natural labour, a constant resort to them early in the first stage, generally indicates other than the ordinary pain, and particularly when the pains come on more rapidly than usual. With this there is more frowning, more active contortion of the features, biting of the lip, and, what is very characteristic, violent straining or forcing ; not three or four prolonged efforts, as in the expulsive stage of labour, but in a series of short spasmodic jerks, almost involuntary, attended with great pain and expressions of impatience and suffering. The patient will often say she never had such pains before.

Frequently the patient, misled by the severity of the pains, their "forcing" character, and by the sensation of fulness and distension in the lower part of the vagina, imagines that the labour is near its termination, and betakes herself to bed at an unusually early period.

If the hand is placed on the abdomen, it will be found that the increased firmness of the uterus indicating a commencing contraction is at once followed by spasm of the abdominal muscles, and an examination per vaginam shows the same thing; first, tension of the cervix uteri, or of membranes, if they can be felt, from the action of the uterus, and then the uterus itself is forced downwards into the pelvis by a series of spasmodic contractions of the abdominal muscles. When this takes place, the normal action of the uterus appears to be suspended, as if replaced by the abnormal actions induced, or it operates under great disadvantage. At any rate little dilatation of the os uteri is effected; the labour is greatly protracted, and the patient suffers much unnecessary pain.

There may or may not be symptoms directly calling attention to the bladder; frequent desire to pass water, or inability to do so, or both, and there may or may not be accumulation of urine in the bladder. Sometimes, also, prolapsus, with distension, may exist without giving rise to any special symptoms in the early stages of labour.

The prolapsed condition of the bladder is readily recognised on examination, especially when it contains urine in any considerable quantity. The cavity of the pelvis is found to be occupied by a bag of fluid easily distinguished from the fœtal membranes, by the fact that it springs from the pubis, and does not permit the finger to pass between it and the symphysis. As this sac, the bladder, fills up the hollow of the sacrum, the os uteri cannot be reached till the urine is evacuated, and if this is done by the catheter, the instrument can be felt from the vagina, and followed to every part of the bladder. When the bladder is perfectly empty, the displacement may be overlooked, but the finger, instead of circumscribing readily the lower segment of the uterus, meets anteriorly with the bladder passing from the symphysis pubis to the uterus, and usually disposed in rugæ; the introduction of the catheter at once makes the case clear. The os uteri is almost invariably found high up and far back, so that the anterior part of the lower segment of the uterus, which may be felt in the vagina, and along which is spread the prolapsed bladder, presents an unusually extensive surface. I am disposed, indeed, to believe that the displacement of the bladder is for the most part secondary to the displacement of the uterus.

Partial prolapse I have considered worthy of mention distinct from the complete displacement, because it is more likely to be overlooked, and because the symptoms, though nearly the same, are usually even more severe. There is almost constantly a frequent desire to pass water, and the urine never accumulates. On examination, the lower part of the posterior wall of the bladder is felt in firm, small rugæ behind the pubis, generally tender when touched, and the seat of extreme pain when the uterus is forced down into the pelvis. The catheter may be felt to pass up between the cervix uteri and symphysis pubis, showing that the prolapse is not complete. The pains have the spasmodic straining character before described; the uterine contractions affect the displaced bladder, causing pain and irritation; as a consequence, violent reflex action of the abdominal and perineal muscles is set up; a kind of tenesmus, which supersedes the normal uterine action, and thus not only is the first stage of labour rendered long and exceedingly painful, but the patient is more exhausted than by a prolonged first stage without this complication.

I have enumerated as other conditions of the bladder affecting the first stage of labour in a similar manner, distension while *in situ*, and "irritability." Cases will be given which, I think, justify me in including them, and I pass them over without further remark than that the symptoms are of the same character, but usually not of the same degree of severity.

The treatment in these cases is simple, and often very effectual. Whenever the symptoms I have mentioned arise, pain in the region of the bladder, with premature involuntary straining, attention should be directed to the bladder, and I think it best always to introduce the catheter. Should any accumulation of urine be detected in the vaginal examination it is absolutely necessary. The patient should also be placed on her back; this not only relieves the bladder from a certain degree of pressure by the falling back of the uterus, but in those cases (and I believe they form a large proportion), in which there is an unnatural position of the fundus of the uterus forwards, and of the cervix backwards, the supine position will aid in bringing the axis of the uterus into correspondence with the axis of the brim of the pelvis. In some cases, also, much good may be done by drawing the anterior lip of the os uteri downwards and forwards, during an interval, and fixing it during a pain in this position. A third means, to which I should unhesitatingly resort, is the administration of chloroform. The effect is to put a stop to the violent spasmodic action of the abdominal muscles, which is of a sensori-motor character, while the proper uterine contractions, being not interfered with, neither by the straining effort, nor by the anæsthetic, effect the dilatation of the os uteri. The head once engaged in the pelvis, the displacement of the bladder, though it may still cause great suffering, does not interfere with the progress of the labour, provided, of course, that accumulation of urine has been guarded against.

I may here introduce the few remarks I have to make on the influence of position in labour. The advantages of the supine position in cases of relaxation of the abdominal wall with falling forwards of the uterus, having long been recognised, but they are not confined to instances in which this condition exists. Delay in the going up of the anterior lip of the os uteri perhaps more commonly marks the fact that the descent of the head into the pelvis is slow, than itself operates as a cause of obstruction to this descent. It will always, however, be an advantage to have it out of the way, and this may often be effected by placing the patient on her back. Any one may observe, as I have often done, the difference which this change of position from the side to the back, makes in the position of the anterior lip of the os uteri, and many members of the society may have seen cases in which it has been attended with a marked change in the rate of progress of the child through the pelvis. One case also I have seen, in which, from the falling of the uterus towards the left side, the head of the child seemed to be directed against the right side of the pelvis, there being further a slight bending in of the ischium on this side; after long waiting, this condition suggested the placing of the patient on her right side, the supine position having been tried with no effect; the head at once cleared the obstacle, and the labour was speedily terminated, without any apparent increase of force in pains.—*Obstetrical Transactions, Vol.* 5, 1864, *p.* 45.

111.—READY-MADE PLASTERS.

Dr. Tilt drew attention to the fact that when a hundred or a hundred and fifty grains of common starch are boiled in an ounce of glycerine, the result is a very stiff glutinous compound, which has no smell, and does not become rancid; and although sticking firmly to the skin, it can be removed and reapplied. Instead of ordering belladonna plaster, Dr. Tilt prescribes three grains of sulphate of atropia to be rubbed down with a few drops of glycerine, then incorporated with an ounce of hard glycerine ointment, and thickly spread by the patient on gutta-percha cloth or impermeable wash-cloth. This can be removed for the morning ablutions, and reapplied after spreading a little more ointment on the same plaster. Morphia and other alkaloids are prescribed in the same way. The samples exhibited by Dr. Tilt were made by Mr. Bullock, of Hanover-street.—*Obstetrical Transactions, Vol. 5, p.* 41.

112.—ON THE USE OF CHLOROFORM IN PUERPERAL CONVULSIONS.

By Dr. Charles Kidd, London.

While studying recently the general phenomena of chloroform administration in midwifery, more especially the stage of convulsive excitement that usually precedes the stage of deep narcotism, attention has been drawn particularly to a physiological fact of some importance—viz., that black venous blood in muscles, or pressure exerted on the *venæ cavæ*, always increases this condition of chloroform excitement; in a word, that black blood acts as an abnormal stimulus to muscles, and most probably such pressure as that of a gravid uterus on the cavæ and other venous vessels in the abdomen in some patients may be one of the causes, or, more correctly, the immediate or chief cause of one of the forms of puerperal convulsions; very possibly that form said to be so much relieved by bleeding from the arm, where this back tide or *remora* of venous blood is taken directly out of the system. Of the fact physiologically there is no doubt, that such convulsion and black blood in muscles form impediments to respiration and are almost inseparable from chloroform administration! The diaphragm, as one of these muscles (and an important one), is not free from these convulsive actions; the cardiac orifice of the stomach too, which passes immediately through a foramen of this muscle, and is always, like the muscle, intensely sensitive to such pressure or convulsion (though the pyloric end is not) both react—hence the violent vomiting so often under chloroform, or vomiting and hiccup occasionally in ordinary labour cases, where the labour is rather slow—hence probably the misery of "sea sickness," from the up-and-down dragging of the cardiac orifice or end of the stomach in the foramen of the diaphragm—hence probably the "morning sickness" of pregnancy, the enlarging womb pushing up the stomach. The author submits these as reasons why puerperal convulsions *during* labour or *previous* to it are always more tedious or troublesome to deal with than the convulsions which sometimes follow labour. The pressure of the gravid uterus conveyed to an impacted state of the bowels or congested liver, will keep up this form of pressure on the larger veins, and thus throw back a quantity of dark blood into the muscles. The analogy of numerous experiments on the lower animals would also indicate that mere black venous blood of itself is sufficient, the blood need not be poisoned with the elements of retained urea, carbonate of ammonia, &c., as theoretically supposed in manuals and books. Hyperæmia alone appears as a sufficient predisposing cause of convulsions, and chiefly by causing congestion of the muscles, and not, as usually supposed, congestion of the veins or vessels of the brain. The difference is at once apparent, though "standard books," so called, are almost every one wrong on the subject.

The form of puerperal convulsions which is seen *after* a labour, I am anxious to procure the experience of readers whether this is not usually or more often "reflex," induced (as in epilepsy) by irritation of peripheral nerves reflected back to the medulla oblongata and spinal column; thus several cases of puerperal convulsions of late, when carefully examined, were cured by chloroform, and found to be caused by the necessary irritating manipulation of passing a hand into the uterus to remove an adherent placenta. Here the irritation, purely nervous, starts from the nerves of the uterus or vagina, reaches the spinal cord and medulla oblongata, and is reflected back upon these very centres (whether through their nutrient nerves or otherwise, may be a little doubtful and not very important to decide); hence morbidly increased action and puerperal convulsions, which differ in a most marked degree from the previous variety caused by dark blood in the muscles.

Of these curious sympathies or associations of the genital organs in females with convulsive diseases, as seen in passing a hand into the uterus, the author would further cite several cases of epilepsy or epileptoid affections like convulsions, decidedly improved, if not cured, by cauterization or excision of peripheral nerves in irritable parts, as the clitoris, which he has seen during the past two years. A corroborative fact, the converse of this, too, has been noted, that in bad vesico-vaginal fistula cases, or recto-vaginal injury in hospitals, where the normal condition or sympathy of the vagina for a time with the ovary has been disturbed or paralysed, menstruation usually ceases, but the moment the urine, trickling through the vagina, is redirected into its proper channel by an operation, menstruation is set up again with increased facility of flow; tied and bound together thus, so to speak, curiously, are the nerves and nervous associations of the vagina or uterus and its appendages, each acting, as in puerperal convulsions, through the medulla oblongata and spine.

Irritated ovary, associated with constant bleedings and enlargement, simulating fibrous diseases of the uterus, with most marked anæmia (the patient appearing as white as paper), the author has known to resist all routine tonic or steel hospital medicines (which, in fact, made the pain and anæmia worse), but yield in a few days to a removal of the primary cause of the disease under chloroform—namely, excision of irritable peripheral nerves as just mentioned. In 313 cases of puerperal convulsions treated by chloroform chiefly, and given by Sinclair and Johnson alone in their practice, the chloroform has a decidedly good effect in lessening the "impressionableness," so to term it, of the spinal cord. In all these puerperal convulsion cases of a reflex nature, but one is not so sure of the cases where the irritation, so to speak, is mechanical, by a back current of blood or hyperæmia of the muscles. It is hopeless to ask societies, like our Obstetric one in London, to go out of its course to correct any errors as to chloroform, prejudice is now so strong against this agent. It is probable that the use of some medicines, such as opium, tends in no indirect manner to predispose the patient to these hyperæmic convulsions in the puerperal state, not necessarily, perhaps, so much by inducing uræmia as by causing an obstinate condition of the bowels and sluggishness of the liver. We are not now concerned with the exact function of bile or the liver, or if calomel act on the liver (which some are supposed to doubt!) but these congestive cases are perhaps those where five or ten grain doses of calomel have been supposed to do so much good by unloading the bowels and preventing pressure on the abdominal veins.

Some of these nervous associations of the uterus are singular and unexpected, like the pain at the tip of the right shoulder from liver disease, in a severe neuralgic pain well known in hospitals as "hysteric knee-joint;" and Mr. Hilton ascribes it to a nerve which he has demonstrated, which goes to the ovary and sends a long unexpected branch through the obturator opening to the knee-joint and another to its inside! So that even injury of a knee-joint might simulate ovarian derangement or *vice versâ*. There can no longer be any doubt as to the exceeding value of chloroform in ordinary midwifery practice. It is a great charity to the agonized patient in many cases, as when properly administered it need never interfere with the progress of the labour "pains." One hears occasionally, as from my friend Dr. Tyler Smith, of a case of labour which partly stopped while the patient was taking chloroform; but we forget that a labour may stop midway without any chloroform at all. One hears often, too, of women, as in cases of Dr. Greame, rendered deeply insensible for hours together through a labour. This need not be either, as we will find it scarcely ever necessary in labour to narcotise the great reflex centre or destroy consciousness. There are several errors afloat as to parturition without pain, and dilution of the chloroform so as to prevent its entering the blood, but such fictions cannot deceive scientific observers. Chloroform by rigid experiment is found always to enter the blood in inhalation of the vapour, but not at all to be decomposed in the blood under such conditions.

The writer submits whether this great difference between these two varieties of puerperal convulsions may not reconcile the vast differences of opinion that exist, even amongst the most experienced accoucheurs, as to the usefulness of chloroform in puerperal convulsions or midwifery generally. In one class of cases, that from congestion, it can scarcely be expected to act so well, as in the instances where the convulsions depend on abnormal nervous reflex action. It should not be forgotten, indeed, that even in the most ordinary "labour" reflex excitability is much increased by the process of evolution of the child in the uterus, especially if the membranes be once ruptured, when the fœtus becomes, as it were, a foreign body to be then thrown off. If this be subsequently continued by passing the hand into the uterus to remove a placenta, convulsions, as we know, are often excited. I believe that in none of the cases is the blood poisoned as usually supposed. *Dark venous blood alone excites convulsions.* Finally, it has been recently noted in practice of midwifery that chloroform has a tendency to induce a slight form of *inflammation of the lungs* by direct action, as supposed, of the chloroform as an acrid irritant on the air-vessels, but I have never seen this inflammation in hundreds of patients who had had chloroform in hospitals administered for various operations, nor in a considerable experience of midwifery patients who had had the same anæsthetic during labour. I am inclined to believe that in one or two patients, for instance, where it was seen, it was caused by unnecessary deluging the patient's chest with ice cold water as a restorative from apprehended syncope. Bad chloroform no doubt acts, when taken internally, as a violent irritant or narcotico acrid, and externally blisters the skin. Impure acid chloroform may excite the delicate bronchial capillaries also; but pure chloroform has not been observed to do so, except possibly where slight bronchitis already existed, or the patient's chest was deluged with cold water, which it ought not to be.

The ultimate practical conclusions to be deduced from direct experiment on animals—viz., that convulsions ensue where mechanical pressure is made on the venæ cavæ, as also from the almost invariable concomitant of chloroform administration (where respiration is impeded), that black blood collects in the veins seems to be—

1. That similar pressure of a mechanical kind on the abdominal veins by a gravid uterus is one of the causes, or more correctly, the immediate or chief exciting cause of one form of puerperal convulsions, the true remedy being to empty the uterus and remove the mechanical pressure.

2. That another very marked form of puerperal convulsions is caused by such irritation as that excited by passing a hand into the uterus, and here chloroform administration is eminently beneficial; and,

3. That there is probably no real blood poisoning in these convulsions, as theoretically supposed, in books copied from one to the other, the carbonate of ammonia or urea leading to coma, &c., but rather a state of simple hyperæmia or *remora* of black blood into the muscles in one class of cases, and simple reflex irritability of the uterine organs in the other, as now abundantly proved by sundry forms of convulsive diseases so superinduced, but only recently studied since the introduction of chloroform, and of clearer views on epileptic, convulsive, and other allied forms of uterine irritation.—*Dublin Medical Press, March* 2, 1864, *p.* 219.

113.—CASES OF VASCULAR TUMOUR OF THE MEATUS URINARIUS.

Under the care of Mr. Shillitoe, Surgeon to the Lock Hospital, with remarks by Dr. G. de Gorrequer Griffith, late House-Surgeon.

There is at present under the care of Mr. Shillitoe, among the out-patients, an old and infirm married woman (about seventy years of age), who has for several years been a sufferer from the effects of vascular tumour of the meatus urinarius.

Sometime since she presented herself complaining of great pain in the genital organs, almost persistent—that is, she scarcely ever was quite free from it. By the efforts at micturition this pain was so intensified as to make it amount to excruciating agony, and that, not alone during, but for sometime subsequent to, the performance of the function, so that the bladder was kept unemptied as long as the patient could, without actual pain, retain the urine; the urine itself was "thick and muddy" (to quote her own expression); was somewhat ammoniacal; as she entered the consulting-room, there was a distinct urinous odour given off from her person, and which, no doubt, was occasioned by a kind of stillicidium urinæ, she being not at all times able to command its retention. She had become emaciated and dyspeptic, fretful, cross, and "quite out of sorts;" there was pain on walking and moving, and a peculiar shuffling or waddling gait, as well as a cautious manner of sitting down, which had been acquired since the onset of her ailment; there was a whitish discharge from the vagina, apparently, however, limited to its lowermost portion, and a creamy or curdy exudation on and about the labia, also a great increase in the urethral mucus; there were likewise pains referable to the back, loins, and down along the thighs; the bowels were usually constipated, and required medicine to cause their relaxation.

Immediately on raising the clothes and separating the labia, some of the above-cited evidences of disease were detected, in addition to a large vascular growth, which seemed altogether to block up the entrance to the meatus. The growth had caused the meatus to have become so very much enlarged in calibre, as to be now quite equal in its area to the size of the tip of the little finger.

This tumour protruded somewhat from the lips of the meatus, which were thus in a measure pouted and everted, at the same time that they were surrounded by an œdematous condition of the mucous membrane; it was seen to be of a bright, shining, red colour, when the mucus, which was poured out over it abundantly, was wiped away with a piece of lint; it was distinctly lobulated, exquisitely tender, and ran up apparently so far as to involve the entire length of the urethral channel, though it seemed to have originated upon the lower wall; it did not easily bleed, and had for its covering the same delicate membrane as lined all the structures of this region; it was not pediculated, nor had it the jelly-like appearance which these tumours so often present; on the contrary, it was firmly consistent, if not even somewhat hard.

Passing the forefinger up along the anterior wall of the vagina, the entire urethra could be felt to be occupied by this growth; but not this condition alone was present. There was hypertrophic thickening of the mucous membrane enveloping the meatus, and also of the cellular tissue surrounding the urethra, conditions in themselves which, we know, and as Sir C. Clarke has demonstrated, would suffice to occasion no small uneasiness or even distress.

The patient being in a depressed weakly state, was put upon tonics, and when the health was improved the growth was freely destroyed by strong nitric acid.

According to Lee, Clarke, Boivan, and Dugès, these tumours are nothing more than a congeries of vessels, encased in their peculiar cellular tissue and in their proper mucous covering.

They may be fibrillated, or even more or less composed of fibrous tissue.

In the ninth volume of the 'Edinburgh Monthly Journal' there is given, by Mr. Queckett, the following microscopic description of one of these tumours:—"The tumour was white, and had numerous small confervoid filaments attached to its outer surface, from having been some time in water. A thin slice, from the external surface, when examined microscopically, exhibited the same structure as ordinary cuticle, the epithelium of the outermost layer being composed of flattened scales; while the cut surface exhibited the same kind of cells more condensed, and firmly adherent together. A vertical section through the middle of the mass showed several papillæ of various sizes, which were very vascular and surrounded by an investment of cuticle, which, with the papillæ, made up the entire mass of the growth; at the part where the papillæ were situated, the growth was smaller than at the opposite extremity, as though it had been attacked by a constricted neck or pedicle.

The papillæ were no doubt largely supplied with nerves, as well as with bloodvessels, "but their presence could not be detected by the microscope."

Dr. Fleetwood Churchill says, "these tumours may be considered as a variety of epithelioma."

The attention of the profession was first drawn to this affection by Mr. Sharp in 1750, and in the year following by Morgagni, who mentions having detected in the orifice of the meatus "a triangular excrescence." The patient of the former was a virgin. The growth was made out during life; the subject of the affection noted by the latter was an old woman, and the existence of the tumour was discovered at the autopsy.

Mr. Hughes, of Stroudwater, Gloucestershire, in a case that came under his notice about the year 1768, excised the meatus along with the tumour; this mode of treatment effectually removed the disease, and there was no return in any remaining part of the urethra.

The treatment pursued has been varied in the hands of different practitioners. Dr. Churchill, *until lately*, preferred excision, and the immediate application of the strong nitric acid, "as being quicker than any other plan of treatment, and less painful." "I think, too," he says, "that the tumour is less liable to grow again, if the caustic be applied freely to the *raw* surface."

"If," he continues, "excision be determined upon, the tumour should be snipped off with a pair of scissors close to the mucous membrane, and the root touched with lunar caustic, nitric acid, or the potassa cum calce.

"The operation occasions some pain, which soon passes off, and it is seldom followed by hemorrhage to any amount; however, I have seen the removal of a small excrescence followed by very considerable hemorrhage; the patient was very unmanageable, and I suspect that the nitric acid was not effectually applied. The bleeding was promptly arrested by a saturated solution of the perchloride of iron in glycerine.

"After a few days the caustic should be reapplied to the root of the tumour, and repeated at intervals until the disposition to reproduction has entirely ceased.

"For some time past, however, I have adopted a modification of this treatment, which appears to me a great improvement. In the first place, I seize the tumour close to the root with Dr. Wilde's instrument for polypus of the ear, which occasions no bleeding, and strangles the tumour; then I snip it off below the instrument and apply caustic to the stump. After the slough has separated, I repeatedly cauterise the root with a red hot knitting-needle until it is entirely destroyed. This is far better than caustic of any kind, inasmuch as no damage is done to the neighbouring parts; we can reach far down the urethra, and the pain, though severe at the moment of contact, is of much shorter duration.

"After the tumour is removed and the caustic applied, the parts ought to be kept constantly wet with some refrigerating lotion, as a means of preventing inflammation and the reformation of the tumour. It will be necessary for the patient to take two or three doses of purgative medicine, and to remain very quite for some days."

Mr. Bringham, of Lynn, adopted, and with success, the practice of touching the extremity of this vascular growth with the actual cautery.

Sir C. Clarke formerly used the broad ligature as the means of removal, but latterly put into practice the plan of excision, and the immediate touching of the raw surface with caustic.

Dr. Ramsbotham uses a thin ligature for the same purpose. Mdm. Boivan, having excised the tumour, applies to the bleeding surface only some powdered alum. Dugès advises simple astringent lotions.

Some adopt the ligature of dentist's silk (waxed), and having strangulated the tumour, remove it, thus strangled, below the point of deligation; others there are, again, who adopting the plan advocated by Mr. Bringham, merely use the actual cautery.

In the diagnosis of the symptoms of which our patient will complain, we must be careful not to be led into the supposition that the uterus, the rectum, or the bladder itself, is at fault, that there is any actual organic disorder of these viscera, and we should be able to recognise the mere sympathy of the organs mentioned from any morbid changes which may affect them, In the case of this vascular tumour of the meatus, there is merely a disturbance of their normal action. Respecting the the time of life at which the affection obtains, Dr. Churchill says, "I saw one in a lady of twenty, and another in one of seventy years of age. They also sometimes occur in very young subjects."

Mr. Norman has seen it exist in patients at the ages of fifty and fifty-two, while Sir Charles Clarke says, that he never had in his experience seen it in any female who had passed the middle age.

I have myself seen it in one instance, where the child was about ten years of age; in another, where the age was about nine; and in a third, where the age attained was only a few months. In all three cases there was vaginal discharge, vaginitis, and urethritis; in the child of ten years there was a syphilitic constitutional taint inherited from the parents, and the mother was, at the same time as her little daughter, suffering from vaginal discharge. In the case of the second child, there was a true and aggravated form of gonorrhœa, produced by a rape committed upon the child. The third child had congenital syphilis in addition to the vulvitis and this vascular growth. They were all treated topically with simple lotio plumbi, under which the local condition slowly amended.

Causes.—I have seen the growth very frequent amongst prostitutes, amongst those who are guilty of masturbation, especially if the urethra have been the site of much irritation and titillation—also in those affected with gonorrhœa, vaginitis, and urethritis;—the latter condition seems particularly to predispose to it, and apparently in the following manner occasions it:—As a result of the inflammation of the mucous membrane, there is increased vascularity of the part so inflamed, the vessels become in a certain measure paralysed, dilated, congested, and engorged; then their coats become organically altered, being thickened by the inflammatory exudations, the areolar tissue in which they are embedded likewise participates in the thickening, and thus increases the amount of prominence. In very many instances I have noticed an œdematous condition either of the mucous membrane covering the surface of the growth or surrounding it, and in immediate continuity and contiguity with it; indeed this œdematous state may even sometimes, I apprehend, be the first stage of the affection, the great vascularity, tension, and excessive sensibility being subsequently developed.

In one case the vascular growth was co-existent, at the time of my examination, with a calculus in the bladder, but had originated after the stone had been formed, as though the latter, acting mechanically—as, in fact, a mechanical irritant—upon the delicate mucous membrane of the urethra, had induced the former.—*Medical Circular, April* 27, 1864, *p.* 276.

MISCELLANEOUS SUBJECTS.

114.—ON THE PREPARATIONS OF OPIUM AND THE SALTS OF ITS ALKALOIDS.

By Dr. A. B. Garrod, F.R.S., Physician to King's College Hospital.

Before going more into detail, I will first take a rapid glance at the components of the drug, upon which its value depends. Morphia is, *par excellence*, the important principle of Opium; almost all the good effect of the remedy may be obtained from its employment. It certainly possesses its anodyne and soporific powers; still, a very slight examination of the composition of Opium will show that it cannot be the sole active ingredient. Good Turkey Opium contains about 12 per cent. of Morphia, estimated as the hydrochlorate of the alkaloid; it follows, therefore, that if the whole power were dependent on this principle, we should find it much more potent than it really is; the hydrochlorate should, in fact, be about eight or nine times the strength of the crude Opium, whereas it is scarcely four times as strong. This is a point to which I have paid some attention, and, in order to determine the relative powers of the two remedies, have selected patients suffering from pain of a chronic character or habitual restlessness,—patients who were intelligent, and whose veracity could be depended upon. To these I have administered, at one time Opium itself, at another Hydrochlorate of Morphia, without the patient being at any time aware of the alteration from one to the other. From numerous trials made in this way I have become satisfied that Hydrochlorate of Morphia is about four times stronger than Opium, both as an anodyne and as a soporific.

We must hence conclude that Opium contains active principles or ingredients other than Morphia.

Let us examine the action of some of the other constituents.

A second alkaloid—*Codeia* —exists in Opium in very notable quantities, and is prescribed, both in this country and more especially in France, as a substitute for Opium and Morphia.

It is asserted by some to possess the soothing properties of the latter drugs, without giving rise to the uncomfortable symptoms so often arising from their administration. I will give the results of my experience of the action of this alkaloid.

Perhaps many of my hearers may remember that not long after the first employment of the Hydrochlorate of Morphia as a remedy, and when it was known to be accompanied with the Hydrochlorate of Codeia, that the irritation of the skin occasionally witnessed after the use of Opium preparations was ascribed to the presence of Codeia; that such an opinion is erroneous can be easily demonstrated, for when Hydrochlorate of Morphia is completely freed from Codeia, as it is in commerce at present, it still produces irritation of the skin in certain individuals; and again, when a salt of Codeia is given alone to the same patients, no such symptom is manifested: this I have proved by clinical observations. We have next to inquire whether Codeia produces the good effects of Opium or Morphia. I have endeavoured to test this in several cases. In one instance, that of a patient having a tumour in the orbit, giving rise to neuralgia in the branches of the fifth nerve of daily occurrence, I had a good opportunity of putting this question to the test. I may state that half a grain of Hydrochlorate of Morphia always allayed the pain, and, in fact, relieved it completely for a time. This having been proved by repeated trials, I proceeded to give the same amount of the Hydrochlorate of Codeia; but its administration was not followed by any appreciable effect upon the pain. In other trials it was increased until five grains were administered, but still there was an absence of relief. Similar trials upon other patients have led to the same result; so that I feel quite satisfied that Codeia does not possess the anodyne powers of opium or morphia. To test the soporific properties of Codeia, I have given it in doses varying from one to three grains in cases of restlessness or wakefulness in which morphia has previously been shown to prove efficient, but in every case have failed to observe that any powers of inducing sleep were manifested. From my own clinical experience, therefore, I am forced to conclude that, although Codeia does not give rise to any of the disagreeable symptoms of morphia or the ordinary preparations of opium, yet, at the same time, it is devoid of their valuable properties; and, in fact, that it is a drug possessing slight, if any, known curative powers; whether it has any marked influence upon the spinal system, is a point perhaps worthy of consideration.

Another substance, Narcotine, a nitrogenised principle, contained somewhat largely in opium, deserves a few words. When first discovered, it was supposed to be the active ingredient of opium; to it was afterwards ascribed the stimulating properties of the remedy, but it has since been shown that both these assertions are altogether erroneous, and that, if it possesses any remedial powers, it acts rather as a tonic and antiperiodic. The late Dr. Roots at St. Thomas's Hospital, demonstrated its inertness as an anodyne and soporific, and I may add that I have repeatedly given from ten to thirty grains of the Tartrate of Narcotine without the production of any symptoms.

The peculiar acid of opium, Meconic Acid, is likewise devoid of any detectable remedial or poisonous properties.

To what then can be ascribed the excess of power which opium possesses over its contained morphia, seeing that neither the codeia, the narcotine, nor the meconic acid exert any appreciable influence? We must remember that opium is very complex in its composition, that it possesses many other alkaloids and neutral principles, whose action on the system is not understood; and besides these, there always exists in opium a considerable quantity of resinoid matter, and to this latter substance much of the difference between opium and morphia is doubtless to be ascribed.

These Resins, for they are numerous, are more or less soluble in water, especially if the solution be acid, as is the case with that of crude opium; they are also soluble to a much greater extent in spirit, and hence the various pharmaceutic preparations of opium are more or less impregnated with them. I have had several opportunities of giving these resins, and the symptoms exhibited have been giddiness, nausea, and strongly contracted pupils without any very marked sleepiness; in fact, many of the effects which at times render the administration of opium very objectionable. I should feel very inclined to attribute much of the difference in the effects of opium and morphia to the presence of the resinous matter in the former. I speak, however, with some diffidence on this point, knowing that all therapeutic observations require to be frequently repeated before the results can be confidently relied upon.

Having premised these few remarks on the constitution of opium, and on what is really known of the action of its several constituents, we are now better prepared to discuss the preparation contained in the British Pharmacopœia. You will observe, and, perhaps, be surprised to find, that the only preparation of morphia introduced is the hydrochlorate, and that the acetate, a salt so long employed and so prized by some physicians, has been omitted. Why has this been done? We should certainly have some good reason for omitting this old-established remedy. Can we prove that it is unnecessary to retain it in the Pharmacopoeia? I believe we can. As I do not know of any other clinical observations on the relative powers of the different salts of morphia except those which I have had opportunities of making, I must trespass on your time for the purpose of giving you the results which I have arrived at. There are many Medical Practitioners who certainly consider that each salt of morphia has peculiar powers; from what I have gathered in conversation and otherwise, I find that some look upon the acetate of morphia as possessing the property of determining powerfully to the surface; upon the sulphate as being much more of an irritant than the acetate or hydrochlorate; upon the meconate (the salt existing in opium itself) as being milder in operation than the rest; and so on for the various other combinations of this alkaloid. On more closely questioning, however, the grounds upon which such assertions have been made, I failed in every instance to discover their validity; those who advocated the advantages of the acetate, had made no real or sufficient trials of the hydrochlorate, and the same remarks equally applied to the advocates of the other salts of morphia. I have carefully and clinically examined this subject, and will state my evidences and my conclusions.

I have been particular in selecting the subjects of these observations, and, among others, have chosen the man before referred to, suffering from neuralgia of the fifth nerve due to the pressure of an orbital tumour, and without exception I have failed to discover the slightest difference in the action of the acetate, hydrochlorate, sulphate, meconate, tartrate, or phosphate of the alkaloid; but in all these observations I have taken the precaution of giving exactly the same amount of the real alkaloid, not of the salt, for in the various saline combinations a different per centage of morphia is contained. In no one instance could the patient tell which salt was administered, or find any difference in the operation of the medicines. If we reflect for a moment, should not we naturally expect such a result? Suppose, for instance, we give half a grain of the acetate, can we reasonably look for any appreciable effect from the trace of acetic acid therein contained; and the same remark applies to the minute fraction of a grain of hydrochloric, sulphuric, meconic, tartaric, and phosphoric acids, contained in the ordinary doses of their morphia salts; given alone, these small doses of the acids would be absolutely inappreciable, even, I should imagine, to the eye of the homœopathist; how, therefore, can we expect that when in a state of combination their peculiar actions can be detected by the Physician?

If these statements be correct, why should we have in the Pharmacopœia more than one salt of morphia, and, granted that one is sufficient, our object should be to select a salt which possesses properties which peculiarly recommend it.

The Hydrochlorate has these qualifications. It is a well-known salt; has been long employed; is readily manufactured; forms very beautiful crystals, of a definite composition; and is not liable to change on exposure to air; whereas the acetate is very difficult to crystallise, and is a salt liable to decompose, to lose some of its acetic acid, and to become much less soluble. These are the facts of the case, and probably the consideration of such led to the rejection of the acetate of morphia from the British Pharmacopœia.—*Medical Times and Gazette, March* 26, 1864, *p.* 333.

115.—ON LETTUCE AND ITS DRIED JUICE LACTUCARIUM.

By Dr. A. B. Garrod, F.R.S., &c.

[Neither the lettuce nor its dried juice lactucarium, which were contained in the London and Edinburgh Pharmacopœias have been included in the materia medica list of the British Pharmacopœia.]

The lettuce, more especially the wild plant Lactuca Virosa, contains, as you are doubtless aware, a milky juice, which is exuded when the stem is punctured, and this exudation, when it becomes dry, forms a brownish substance, having a peculiar narcotic odour, not very unlike opium, and a very bitter taste; these tears or masses pass by the name of Lactucarium, a preparation in the Edinburgh and Dublin Pharmacopœias; an extract obtained from fresh juice of the plant, under the name of Extractum Lactucæ is in the London Pharmacopœia.

There is a very general idea that the eating of the lettuce by wakeful patients predisposes to sleep; also that the preparations of lettuce have powers of allaying irritation, such as troublesome cough and so forth; and the Lactucarium was thought to possess all these properties in an exalted degree, and has been frequently substituted for opium for patients in whom this latter medicine produced disagreeable symptoms.

Have these preparations any remedial powers which entitle them to a place in the Materia Medica? This is a question which I will endeavour to answer, or at least I propose to lay before you certain facts or results which I have recently obtained in the clinical investigation of the action of these medicines. In arriving at these results, I have selected patients either suffering from want of sleep or labouring under spasm, more especially spasmodic cough, or individuals afflicted with painful disorders. The usual dose of Lactucarium, or of the Extract of Lettuce, is, I believe, from five to ten grains. I have carefully observed the action of such doses, but failed to find any influence either in producing sleep, allaying spasm, or alleviating pain. I afterwards gradually increased the amount of the preparations until at last as much as sixty grains of Lactucarium or of the Extract of Lettuce were administered at a time; and these doses have been repeated three and four times during the day. In no one instance did the Lactucarium or Extract of Lettuce produce any direct tendency to sleep, nor in cases where pain was present was relief obtained. These observations were repeated frequently at different times, and on several patients, male and female. At first, on mentioning these results, I was told that the preparations I made use of must be faulty. To assure myself on this point, I made further trials with drugs which I felt sure were the best that could be obtained; but the results of these further observations were exactly the same as those obtained at first; and hence I cannot help doubting the value of Lettuce in the treatment of disease. It is supposed by some that Lactucarium may be usefully employed in cases where the stimulant effect of opium is objectionable. That it is devoid of the injurious effects of opium I am quite willing to concede, but that it possesses the valuable properties of that medicine my own clinical observations lead me to question.—*Medical Times and Gazette, March* 26, 1864, *p.* 334.

116.—ON CERTAIN ABUSES OF CAUSTICS.

By Dr. James Morton, Lecturer on Materia Medica, Anderson's University, and Surgeon to the Glasgow Royal Infirmary.

[Dr. Morton considers that the use of caustics is often abused, and he thinks such errors may frequently be traced to directions contained in the works of some of the leading writers of the day. We find them frequently employed in some diseases affecting the whole system, over which caustics, as such, can have no influence whatever.]

In common inflammation of the fauces, popularly known as sore-throat, and usually ascribed to cold as a cause, and whose symptoms I do not take time to enumerate, presuming that no one can mistake it, surely the employment of caustics cannot be said to be requisite. But besides being unnecessary, there is the additional objection that it is the means of inflicting a very considerable amount of pain. The disease will speedily disappear, if left to itself, or treated by soothing agents. Should suppuration take place, the possibility of the caustic application producing an earlier evacuation of the pus must be admitted. This, however, must be regarded as an accidental circumstance, as the caustic is not employed with this intention.

Very much the same may be said in respect to ulcerated sore-throat, except that in addition some mild alterative may be required, and a caustic application can only rarely, if ever, be necessary to repress prurient granulations, as in other parts of the body.

In regard to the three eruptive diseases, measles, scarlet fever, and small-pox, in all of which the throat is so apt to become affected, it is difficult to speak so as to avoid misconstruction. When a slough forms upon the fauces, the part is often diligently assailed by the over-zealous practitioner, in the hope of thereby arresting the sloughing process; and when it ceases to extend, he is thought to have succeeded, and he plumes himself and is praised by others accordingly. I have no hesitation in affirming that this gratulation is often misplaced, and that the slough would have been smaller, and would more speedily have disappeared, had no caustic application been resorted to. This is not unfrequently exemplified in the treatment of scarlet fever, in which the mucous surface of the throat often suffers severely. Almost every one who has seen much practice must have witnessed cases of this severe throat complication occurring in children, not infants, who have obstinately and perseveringly, and with success, resisted all attempts to cauterize their throats, or even to touch them. In very many instances these do as well as, or better than, those who may have been subjected to the ordeal; and the former are often as severe in the character of the attack as the latter. The more rarely, therefore, that we employ caustics in such cases, the better for our patients, and the more pleasant will be our treatment. All the local applications may be of a soothing nature, and it is not our present duty to discuss the constitutional treatment. (It may here be proper to remark also, that the ectrotic treatment of small-pox by nitrate of silver is a topic foreign to the object of this paper). Some have an idea that active treatment of the local complication has a powerful influence over the constitutional affection; or, in other words, that the speedy removal by caustic applications of the morbid exudations from the situations upon which the disease seems to fasten in its greatest intensity, is of the very greatest effect in counteracting the deleterious action of the morbid poison upon the general system. This notion (it is scarcely entitled to be called an opinion) will not be maintained by many, and is such an untenable position that it does not seem to be necessary to attack it. An opposite mode of reasoning is taken by the majority, viz., that the severity of the systemic poisoning has much to do with the intensity of the local manifestations of it.

After what has now been said with more especial reference to scarlatina, it is not requisite to say much respecting measles and small-pox. Affections of the throat are not so common in these two varieties of the exanthemata, and the objections already urged may apply to rubeolous and variolous cases in which the throat is found to suffer.

In the disease now styled diphtheria the use of caustics, as they are too often employed, is, in my opinion, productive of the most disastrous consequences. All the symptoms of this malady indicate the presence of a general toxic agent, probably an epidemic poison, and the prominent symptoms are those of debility, the throat affection coming on insidiously, often unperceived for a time, often with little or no pain, and a slight degree of swelling, though in some cases the tumefaction in and around the throat is very considerable. The chief indication is to support the strength; another important though subsidiary one ought to be, to avoid measures calculated to add to the existing local complications. It is a custom with some, I hope not with many, to divest the fauces of the whitish leathery covering which forms upon them, literally to dissect it off, and then to apply the solid nitrate of silver or some liquid caustic freely; and not only so, but to repeat this process daily, or as often as the adventitious membrane reforms, and in the belief that they are only doing what is absolutely necessary towards giving their patient the best chance of recovery. This line of practice I regard as a woeful mistake. It seems to me that, by so acting, the surgeon is diligently endeavouring to undo all that nature is attempting to effect towards a spontaneous cure of the malady. Surely no one imagines, that by tearing the exudation off the fauces, he will prevent its extension into the larynx and trachea. Such a procedure seems to me more likely to promote the dangerous progress of the false membrane, to use a phraseology now somewhat antiquated. To prevent misconstruction, let me add that no one can object to the removal of sloughy matters flapping loosely in the pharynx.

The following history may be taken for what it is worth. I freely admit that it would be rash to use it as the foundation of any argument *pro* or *con*:—

One of the children of a family in this city presented symptoms of well-marked diphtheria, and was treated on the mild or soothing plan—namely fomentations, wine and nourishing food. The next younger child was at this time removed by a relative to a not unhealthy locality a few miles from town, took diphtheria, and there was treated by the application of caustic and the use of mercurial purgatives, with blistering; the result being fatal. The same might have been the termination under any treatment; yet this history is suggestive of inquiry. It is right also to add, that the child which recovered was previously considered the most delicate in the family.

A recent writer has said that the more copiously lymph exudes upon the fauces and larynx, the less likely is it to be deposited along the interior of the bronchial tubes in croup and diphtheria; and the inference from that is that in such instances tracheotomy is more likely to succeed. This assertion we may not all admit, and, at all events, it has not been established as a fact; but though it were, caustics are not used for the purpose of promoting the deposition of lymph, neither will their free application render the success of tracheotomy more probable.

In the belief, then, that caustics in all forms are injurious in diphtheria, I would venture to recommend their complete abandonment in the treatment of this peculiar but perilous disease. I am aware that more heroic treatment still has been proposed in diphtheria, such as excision of the tonsils (as a certain method for removal of the false membrane), tubing of the larynx, and tracheotomy, so as to get under this exudation. It is foreign to my purpose to consider such at present; but to show that I do not stand alone in my opposition to the use of strong caustics in diphtheria, the liberty of quoting the following from Dr. E. Headlam Greenhow's work on Diphtheria, may be accorded me. "Local treatment," says Dr. Greenhow, "applied to the throat internally, has been almost universally adopted in the treatment of diphtheria; and though I by no means deny its value when judiciously employed, I am sure much mischief has been produced by its indiscriminate use, especially by the frequent tearing away of the exudation by probings or similar contrivances for the application of nitrate of silver or of strong caustic solutions. Observing that removal of the exudation, and the application of remedies to the adjacent surface, neither shortened the duration nor sensibly modified the progress of the complaint, but that the false membrane rarely failed to be renewed in a few hours, I very soon discontinued this rough local medication to the tender and already enfeebled mucous membrane. The propriety of this course became evident at the very first post-mortem examination I had the opportunity of witnessing, and has been confirmed by all my subsequent experience. In the first place, the application can but rarely extend to the entire diseased surface; and in the next, the subjacent tissues are so deeply involved in cases of really malignant diphtheria, that any application to the surface of the mucous membrane could apparently exercise no beneficial influence upon the disease. The same observation applies with even greater force to the indiscriminate use of strong solutions of chloride of zinc, or of Beaufoy's solution of chloride of soda. The only instance," he continues, "in which much benefit can be expected to arise from the local application of escharotics, is when the patient is seen at a very early stage of the illness, while the throat is simply inflamed, or the exudation, if it be already present, is circumscribed, fully in view, and surrounded by healthy tissue." For such a case he recommends the tinct. sesquichlor. ferri, slightly diluted, evidently referring to slight cases. In such also he uses the same tincture as a gargle, and recommends milder and more soothing detergent applications, merely to cleanse mouth and fauces; and ends by stating that tepid water may answer the purpose equally well.

The opinions of Dr. Jenner of University College are also well worthy of consideration, though he does not go the length of altogether abandoning caustics. He says that he has formed two decided opinions as to the value of local applications, from the cases he has seen treated by others, which are—1st, "That the single efficient application of a strong solution of nitrate of silver—a scruple to a drachm of water—frequently stays the spread of the exudative inflammation; but that, on the whole, hydrochloric acid and water in equal parts more frequently attain that object. 2nd. That the repeated application at short intervals of these strong local remedies is injurious; I think I have seen serious evil result from their application two or three times a day." A page further on, he remarks—"I am sure I have seen severe inflammation of the pharynx kept up by the repeated daily application of irritants, used to cure the disease which they themselves were occasioning." And again, "I have never seen these powerful topical remedies of use while the parts were much swollen, bright red, and covered with mucus; nay, under such circumstances, I have seen them do harm. Do not tear off the false membrane; to do that is to commit a decided blunder. I have seen it done repeatedly, but never with good effect, and sometimes with decidedly bad results." We find the same opinion mentioned in Bretonneau's "Memoirs on Diphtheria," translated by Dr. Semple, where it is said—"The authors of the sixteenth century agree in reprobating the forcible removal of the false membranes, and also scarifications, together with all roughness of frictions and applications. I have had occasion several times to convince myself of the justice of these precepts, and I have seen the pellicular inflammation aggravated by all kinds of mechanical irritation. When the disease is not arrested in its progress by two energetic applications made at intervals of twenty-four hours, and the signs of the affection of the air passages begin to be manifested, this local treatment offers very uncertain chances of recovery." So much for the antiquity of the idea regarding the non-use of irritating applications.

However improbable it may be that I shall be met with the objection, that my remarks are directed against a practice which does not now obtain, it may be right to state that any one who glances at the weekly and monthly medical journals of the day, will at once be satisfied that such is not the case. In the Edinburgh Medical Journal for October last, there is a lengthy article upon diphtheria, the writer of which advocates the vigorous use of caustics, even to the dropping of it into the larynx by a tube, and congratulates himself on thereby obtaining complete command of the symptoms. It is also worthy of remark that this practitioner uses iodide of potassium in ordinary doses, a mode of treatment previously proposed and employed by Mr. Wade, of Birmingham. The cases thus treated did not speedily arrive at convalescence—not so speedily as most recoverable cases of diphtheria usually do—and it may possibly yet occur to the writer that he may accelerate his cures by confining his medication to the latter remedy, and entirely excluding the caustics. The same writer mentions a family in which he was attending one diphtheric patient, where there were other five inmates similarly affected in some degree ; and when describing their symptoms and treatment, he says—"I had cauterized and sponged with the caustic solution their throats daily, and I had ordered to be taken, for three or four days, ten drops of the tinc. mur. ferri twice daily. Nevertheless, the inflammation of the throat and the effusion of lymph kept slowly but steadily advancing." In a subsequent page, when speaking generally of treatment, he remarks—"If the patient, when I was called, was free from fever, but had the throat affected, I contented myself with touching the lymph with the caustic, and sponging the throat, fauces, and glottis with the same solution, and gave the sol. iod. pot. from three times daily to every two hours, according to the urgency of the case." And again, "Where there was the slightest hoarseness, I never failed also at once to drop the caustic solution into the windpipe." Surely a striking example of the *nimia diligentia medicinæ*, and the steady advance of the inflammation and effusion would surprise no one but the writer.

A careful attention to the current medical literature of the day, will convince almost any one that in some cases recovery is not to be ascribed to the local means employed, but rather that it takes place in favourable cases in spite of these means, which can only have the effect of protracting the period of disease, or delaying convalescence. It is perfectly well known to me, that in holding such opinions I do not stand alone, though I fear they are held and acted upon by but a few, by a minority.

While writing this paper my attention was directed, as previously noted, to a pamphlet entitled "Notes on Clinical Medicine" by Dr. W. F. Wade, of Birmingham, the first part of which relates to diphtheria; and after stating that "local treatment exerts no known influence upon the general course of specific fevers," he continues in a succeeding page as follows:—

"It is contrary to the ordinary rules of our art to interfere with the local development of blood poisons, except for special reasons."

"The faucial exudation of diphtheria is to be considered as the local manifestation of a general disease."

"Interference with it will not prevent its reproduction, nor will it prevent laryngeal complication, nor will it prevent the supervention of grave constitutional disorder. It is, besides, exceedingly irksome to young patients."

"We are justified in interfering with the throat exudation when there is excessive fœtor, or when it is so copious as to interfere with respiration or deglutition—not otherwise."

These opinions coincide so exactly with my own in respect to local management, that I have taken the liberty of quoting them; and it may be added by the way that Dr. Wade recommends, for constitutional treatment, iodide of potassium, iodide of iron, and bichloride of mercury, with bark, as eliminants of the blood-poison.

Lastly, in reference to syphilis less requires to be said: for, while it cannot be doubted that caustics are still too frequently employed, and productive of considerable mischief, yet it must also be remembered that their frequent and indiscriminate employment is not sanctioned by the best authorities on the treatment of this disease.—*Glasgow Medical Journal, Jan.* 1864, *p.* 409.

117.—ON CARBOLIC ACID.

By Dr. James Watson, Physician to the Edinburgh Royal Infirmary.

[Although carbolic acid has been long known it has only been of late used in medicine. We refer our readers to a most able paper by Dr. Crace Calvert, of Manchester, in *Retrospect*, July to Dec., 1863, p. 367.]

It is as an external agent—as a disinfectant, antiseptic, and otherwise, that it is most highly spoken of. Mr. Oscar Clayton of London, and Mr. Turner, honorary surgeon to the Manchester Infirmary, have successfully used it as a caustic in the treatment of carbuncle and of foul sloughing sores. Mr. Turner prefers it in the treatment of severe cases of diphtheria to other caustics, as it action does not generally extend below the surface of the parts affected. Mr. Campbell De Morgan, Middlesex Hospital, states that carbolic acid dissolved in glycerine or glacial acetic acid has been found by him very beneficial in the treatment of lupus; and Dr. Whitehead treats lupus successfully with an ointment made of carbolic acid,—one half-drachm of acid to one ounce of spermaceti ointment. Several respectable surgeons speak favourably of its use as a lotion—one part of the acid to forty parts of water—in all kind of fetid ulcers, gangrenous and offensive sores; and in necrosis it is said to promote the exfoliation of the dead portions of bone. Dr. Calvert states that it is the most powerful preventive of putrefaction with which he is acquainted. He also states that it acts as an anti-ferment, and that he has proved this on an extensive commercial scale,—that it prevents the conversion of tannin into gallic acid and sugar. He also tells us that a few drops of this acid added to a pint of fresh urine will preserve it from fermentation or any marked chemical change for several weeks.

Carbolic acid is soluble in any quantity of glycerine, in twice its bulk of glacial acetic acid, in forty times its bulk of water, and forms with a solution of sugar a nice emulsion.

I shall now proceed to record some observations I have made as to its action in the Royal Infirmary.

I have tried carbolic acid as a disinfectant lotion in two cases in the surgical wards. In the first case, the patient, a young girl about thirteen years of age, was suffering from a sloughing ulcer extending from the knee to the ankle. There was a large amount of discharge of the most fetid character. On the application of the carbolic acid as a lotion, one part of the acid to forty of water, the very disagreeable fetor was completely destroyed. Nothing could be more satisfactory than the complete manner in which the disgusting odour was dissipated. The lotion also acted as a stimulant to the part, which to a certain extent took on a healthy action, but in consequence of the amount of discharge exhausting the already very weak constitution of the girl, amputation was deemed her only chance of recovery, and this was accordingly performed.

The other instance in which I had carbolic acid applied as a lotion was in the case of a young man who had amputation performed at the middle of the thigh for malignant disease. Very soon after the operation, sloughing set in, accompanied with a fetid discharge. On the application of the lotion, the fetor, as in the first case, was most completely destroyed, the discharge lessened, the sloughs separated, and the parts beneath looked healthy. The lotion continued to be used for several days with the best results as far as its disinfectant and antiseptic qualities were concerned, but unfortunately the patient died, never having thoroughly recovered from the shock of the operation.

I next, along with Dr. Smart, made a number of experiments on urine, to test the antiseptic property of carbolic acid, with the following results:—

First Series of Experiments.—1st, To ten ounces of diabetic urine I added *no* carbolic acid. After standing for five weeks I found a dense (one inch deep) fungoid mass on the surface of the urine, and the urine having a well-marked odour of decomposition.

2nd, To ten ounces of diabetic urine I added one grain of carbolic acid. At the end of five weeks there was on the surface of the urine a fungoid scum a quarter of an inch deep, but no odour of decomposition.

3rd, To ten ounces of diabetic urine I added three grains of carbolic acid. At the end of five weeks there was no fungoid mass or scum on the urine, which had a perfectly sweet odour.

4th, To ten ounces of diabetic urine I added five grains of carbolic acid. At the end of five weeks no fungoid mass on surface. Urine sweet, but with a perceptible odour of carbolic acid.

Second Series of Experiments.—To five specimens of diabetic urine of ten ounces each, I added the following quantities of carbolic acid, and at the end of three weeks the following were the results:—

1st, Of carbolic acid one grain. Urine perfectly free from fungoid mass, and odour perfectly sweet, but there was found a slight precipitate.

2nd, Of carbolic acid two grains. Quite free from fungus, but having a deposit, and urine slightly hazy. Free from odour of decomposition.

3rd, Of carbolic acid three grains. Deposit, with the urine hazy, but quite free from decomposition.

4th, Of carbolic acid four grains. Urine quite translucent, with a deposit less in quantity than in the other quantities of urine, and odour of carbolic acid perceptible.

5th, Of carbolic acid five grains. Urine perfectly translucent and quite free from deposit. Odour of urine sweet, with that of the carbolic acid distinct.

These two series of experiments conclusively show, that carbolic acid is a decided antiseptic,—that a very small quantity prevents for several weeks the odour of decomposition,—that a slightly-increased quantity diminishes the amount of deposit, and that a very little more entirely prevents any recognisable change in the composition of the urine from taking place.

I shall next draw attention to a case of favus treated with carbolic acid in the Clinical Wards.

Peter Russell, aged fourteen. On admission, head was found completely covered with favus crust, and on microscopic examination the parasite—*achorion Schönleini*—peculiar to this disease was discovered. The boy's head was shaved and then poulticed for two days. A solution of carbolic acid in glycerine (in the proportion of one part of the acid to twenty-five parts of glycerine) was then ordered to be applied to his head, morning and evening, and to be continued daily.

Before the application of this lotion the entire scalp was of a crimson colour, but under this treatment the colour of the scalp became gradually fainter, and at the end of five weeks distinct tracts of scalp quite free from redness were found, and the remaining parts were much paler in colour. The hair is now growing vigorously, and over the whole surface of the scalp not a trace of the favus crust can be detected.

The boy's general health is much improved since coming into the hospital.

So satisfactory was the result obtained in the last reported case considered, that the same treatment is now being pursued in another case of favus, with this difference, that in this instance the lotion is stronger, being composed of one part of acid to fifteen of glycerine.

The young man, Peter Ward, aged seventeen, has had the disease for nine years. On admission his head was covered with favus crust. The head having been shaved and poulticed, the above lotion was applied as in the former case, morning and evening. After being treated in this way for fourteen days the scalp was seen to be much less red and the hair growing vigorously. At the roots of the hair there are still small circumscribed patches of crimson scalp, but no trace of crust is to be found over the entire head.—*Edinburgh Medical Journal, Jan.* 1864, *p.* 625.

118.—THE CHEAPEST DISINFECTANT.

By Robert Druitt, Esq., London.

I ask permission to recommend to the readers of the Medical Times and Gazette a strong solution of iodine in methylated spirit, as a safe, cheap, and efficient disinfectant. Iodine is 8d. per ounce wholesale, and methylated spirit 4s. per gallon; therefore, supposing 4 oz. of iodine dissolved in a gallon of spirit cost 6s. 8d., allow a like sum for bottles, corks, and profit, and we ought to get for 6d. about 6 oz. of the tincture.

The advantages of iodine are (I believe it was first proposed as a disinfectant by Dr. Richardson) that it purifies solid surfaces as well as Condy's and Burnett's liquids; and that it is also volatile and acts on the air like chloride of lime, without its abominably nauseous odour.

In every house where there are sinks, closets, &c., internally, it is a good plan to disinfect them thoroughly from time to time in hot weather. Dust-bins should be treated with something to neutralise the emanations of the decaying vegetable rubbish thrown into them. When there is sickness in a house, and a *chaise percée* has to be used, it is very disgusting to have the contents taken without any precaution and allowed to waft a perfume all over the house. For all these purposes, for which a *liquid* disinfectant is convenient, I believe my professional brethren will find the tincture of iodine the most handy and effectual.—*Medical Times and Gazette, May* 21, 1864, *p.* 581.

119.—ON BROMIDE OF POTASSIUM.

By Dr. A. B. Garrod, F.R.S.

On the first introduction of Bromide of Potassium, it was thought to be very analogous in its action to the Iodide, although somewhat less powerful; but little, in fact, was known about its powers. About nine years since I made some extensive trials of this medicine, chiefly in hospital practice, and found that, in certain cases of eruptions of the skin, as in syphilitic psoriasis, it acted as a curative agent, or, at least, patients when under its influence lost the affections under which they had been suffering. I was induced to give the bromide in these cases as the patients were intolerant of the action of the iodide. I discovered, likewise, that Bromide of Potassium, when pure, did not give rise to any of the symptoms to which the name of Iodism has been applied. I did, indeed, occasionally notice these symptoms, but this led me more carefully to examine the salts which had been dispensed; and it was ascertained that, with one or two exceptions, the Bromide, as sold in London, contained notable quantities of the Iodide of Potassium. After this, I took precautions to have the Bromide pure in all my observations upon its action, and the results I arrived at may be thus summed up:—

1. It produces none of the irritation of the mucous membranes of the nose and fauces—no coryza.
2. Some patients experience a peculiar sensation of dryness of the throat and neighbouring parts.
3. When given in large medicinal doses, sleepiness or drowsiness, and dull headache were occasionally noticed.
4. When administered in very large amounts, some loss of power was noticed in the lower extremities, which passed off when the medicine was discontinued.
5. The therapeutic action was decidedly what may be termed alterative—that is, it relieved certain forms of chronic disease, as syphilitic skin affections.
6. No marked action was observed upon the skin or kidneys.

Soon after these observations had been made, Sir Charles

Locock stated that he had found Bromide of Potassium useful in hysterical epilepsy, and in other nervous affections connected with uterine disturbance, and I was from this led to make further trials of the remedy, and have found that—

7. Bromide of Potassium exerts a most powerful influence on the generative organs lowering their functions in a remarkable degree.

8. It is a remedy possessing most valuable powers in diseases dependent on, and accompanied by, excitement or over action of the generative organs; and hence it may be given with advantage in nymphomania, priapism, certain forms of menorrhagia, especially that occurring at the climacteric period; as likewise in nervous convulsive diseases dependent an uterine irritation; and lastly, in some ovarian tumours.

9. It appears to produce an anæsthetic condition of the larynx and pharynx, and hence has been usefully employed in examinations and operations of these parts.

Bromide of Ammonium has been lately proposed more especially for the production of the last-named effects, but I am not aware that it possesses any powers superior to those of the salt of potassium. The bromide of potassium may be given in doses of from five grains to ten or even fifteen grains to the adult.

It is curious to observe and compare the physiological and therapeutic powers of three salts so analogous to each other in a chemical point of view—namely, the Chloride, Bromide, and Iodide of Potassium, the first producing but little action unless given in large quantities, probably from its being a normal constituent of the body; the second, the bromide, abnormal to the economy, or existing only in infinitesimal amounts, acting especially on the nervous system; the third, the iodide, also abnormal to the body, having its influence more especially directed to the mucous membranes and secreting organs. The investigation of such actions in relation to the composition of the substances administered may probably one day afford some clue to the comprehension of the effects of remedies.—*Medical Times and Gazette, March* 12, 1864, *p.* 276.

120.—ON CHLOROFORM AND ITS SAFE ADMINISTRATION.

By Dr. Charles Kidd, London.

The increasing importance of rendering the administration of chloroform and other anæsthetics quite safe makes it very desirable at present that the profession should enlarge the circle of its discussion on this subject. It seems such a matter of practical detail and observation by hospital surgeons, rather than of abstract physiological inference or experiment on animals (not under the same emotions and influences as hospital patients) that the author deems it of essential value that the general impression of the Fellows of this society should be obtained.

The large number of fatal accidents indeed of late (so much to be deplored) invests the subject with a degree of serious interest that it had scarcely attained in former years. Several cases have occurred where the surgeon has been put through the painful ordeal of a long cross-examination by a non-medical coroner and jury as to the exact amount of chloroform that is considered an over-dose for adults, as also the necessity of attending to the condition of the heart or not attending to it, the culpability of not watching the pulse, or neglecting some form of special inhaler and the like, ending in a verdict amounting to a censure for malpraxis!

Whereas on these points the Fellows of this and other kindred societies are themselves yet undecided,—what would be an over-dose for one patient being of no effect on another, and the culpability of not attending to the pulse (as to which there was recently a violent controversy in the morning newspapers) being rather an error of hasty generalization when chloroform was first discovered.

One coroner and a London jury indeed, arguing out of a pile of medical manuals and books of surgery (laid on the table of the court by the coroner), especially as to the words "cardiac syncope," prolonged the medical cross-examination for some two hours on these points to the serious detriment of the surgeon's position in his district; and this, added to the misery of having the dead body lying in a surgeon's own house, has painfully impressed the author that such scenes must be prevented by a more exact decision of the profession as to inhalers and their use, the real value of the pulse as an indication of danger, the danger of valvular disease of the heart, so dwelt on in books, the best modes of resuscitation in accidents, &c., &c. "Granting or not granting that the pulse is essential or comes from the *right* not the left ventricle," was the charge to the jury, "we have sufficient evidence the pulse was neglected, nor was the heart examined by the stethoscope, as the books say it ought to have been, nor was the Marshall Hall ready method put in force, which is described in the Lancet as infallible, nor was a silver inhaler of Snow's used, so that great blame is attached to the surgeon."

But it would be about as rational to blame surgeons for the clot in the heart in a case of slow death by croup, or designate it heart disease, as to have said in this chloroform case it was one of heart disease. Yet this was the verdict, and it was generally acquiesced in by many journals with due censure on the medical man. We know, indeed, enough of such verdicts and medical trials of late to be aware that "one such other victory" and one other chloroform accident would have been his ruin. It may occur to any other surgeon any day, the surgeon suffering from no fault whatever of his own, but from the uncertainty everywhere existing as to anæsthetics.

The truth in these conflicting opinions, and uncertainties and crossroads of the science of anæsthetics, the author believes, has been in reality only slowly arriving. Chloroform is a very new subject. It has been remarked by Mr. John Stuart Mill that in all such new inquiries *hasty* generalizations are very common—the rule, in fact, of inquirers—the one particular or last of a series, or the one most striking of a series of apparent causes of an event, is almost certain to be seized on by the mind as its sole efficient cause, though it may in reality be without appreciable effect at all. Thus, it was observed at first that some patients died with the chloroform on a napkin to the face; the result and cause were at once generalized, the napkin caused the death. Again, the pulse (as more easily watched than the diaphragm) was noticed more easily to stop, it was a more striking phenomenon in the operating-room and its hurry. Hence the popular idea of the vast necessity of *watching the pulse;* but it is now found to be as rational to propose to stop a storm at sea by putting up a storm signal. The pulse is in reality only a negative indication of danger, which danger has its real source in the diaphragm and respiration, not the pulse.

Surgery and medicine abound with errors of this kind. Our literature of a periodical kind as to new discoveries has a kind of vested interest in the fallacy of reasoning *post hoc ergo propter hoc.* This is exactly what has taken place in watching the pulse, and coroners censure those who do not do so. It was simply the one most prominent of a series of supposed causes, this stoppage of the pulse.

The fashionable idea or dogma of fatty heart was then adduced as a further explanation; but it has never been shown or demonstrated in a single one of about two hundred deaths now from chloroform that fatty heart or diseased heart existed at all, and also in thousands of cases that valvular disease of the heart does not contraindicate the use of chloroform.

The true explanation of these hasty generalizations as to the pulse and heart, the use of napkins, inhalers, stethoscopes, &c., lies deeper.

It was pertinaciously held in journals that the pulse in these cases is stopped by paralysis; and when it was shown that the left ventricle from which the pulse comes is unaffected in deaths from chloroform but the right ventricle always engorged, then the coroner's jury was told the pulse of course was influenced by or came from this side, left or right, it made no matter. The author has heard this said in court!

The true interpretation of the facts is in reality this. The heart's action is stopped, and its right cavities engorged, not on account of the influence of chloroform on the heart, but its influence on the diaphragm and lungs. *The pulse, if carefully watched, only stops after the diaphragm stops.*

If in one hundred experiments on dogs, rabbits, &c., poisoned by chloroform, we always find this state, *the right cavities engorged*, the statistics (as perhaps all medical statistics) are as finger-posts at the cross-roads of the place to be explored, they point the direction in which what we are seeking lies, but they are not the thing itself. The late Dr. Snow took them for the latter. The statistics said "cardiac snycope," or engorgement from failing action of the heart. We now find, however, that this is not true, an anterior condition had been overlooked—viz., that this gorging of these cavities is only a species of passive back current or *remora*, caused by the lungs and diaphragm, in a state of paralysis or deep narcotism, not receiving the blood from these cavities of the right side. The diaphragm in reality is the first muscle to stop.

The heart, as a matter of every-day observation in experiments, so far from being paralyzed, is passively dilated, but strives actively, but in vain, to push forward into the lungs the large volume of blood arriving through the cavæ at the auricle, and once we establish good artificial respiration the right side is unloaded.

The heart, notwithstanding all the popular fallacies, the author believes is decidedly the last to be paralyzed or to die, the true *ultimum moriens;* and since these views have made progress in medical schools, accidents are more easily prevented; indeed, some first class physiologists now explain the deaths by the opposite of paralysis (Dr. Snow's syncope)—namely, by over-stimulation of the heart!

The cardinal point of so much value in the modern practice of administering chloroform is to watch the breathing of the patient.

The condition of the pulse is negative, or more correctly, it is a reflex indication that respiration is right.

Both are, so to speak, opposite swings of the pendulum on which the regularity of the machinery of life depends during the surgical operation under chloroform, the essential force on the pendulum being the respiration and active state of the diaphragm!

With these general principles as a guide, a few new facts observed of late in clinical practice may be submitted to this Society.

And first, it is most desirable that the patient's head and neck should be free and well exposed, and the top of the window, if possible, open in all operating-rooms to allow ozone or electricity in the open air to get to the lungs.

2. Again, it is of little advantage to strive to rouse up a flagging pulse but through the lungs, by fanning with a lady's fan, rather than the mistake of dashing cold water, which, in fact, depresses the heart.

3. Next, as to the state of the brain under chloroform. Surgical operations are always performed better in the morning than evening. After a night of refreshing and reparative sleep, there is in the morning less chance of nervous exhaustion or sinking. The snoring sometimes noticed under chloroform, the author believes he has observed it to be a result or measure of exhaustion or long continued pain and sleeplessness. The patient goes asleep from relief of pain. This is often very noticeable in midwifery practice. Sleep is not at all the same as anæsthesia. The author thus has seen boys operated on for stone who suffered severe pain for several days previously, but immediately on the chloroform anæsthesia taking effect, to fall fast asleep and snore, and remain in this state for hours. It is probably a condition of deep sleep.

There seems no doubt in delirium tremens patients, or mania cases, a like state of sleeplessness or exhaustion without sleep exists which precludes the safe use of chloroform; but if sleep has been once obtained then chloroform comes in very well. Thus a man of drunken habits may sustain a bad compound fracture and when brought to hospital primary amputation under chloroform be decided on; excited soldiers in the recent battles in America, who have not slept for several days, have come under care with such fractures; but in such cases sleep for a night or two previous to the operation and chloroform is very desirable. The most marked of all contraindications to chloroform is not diseased heart, but delirium tremens or sleeplessness. Many deaths signed "fracture" in the Registrar-General's returns occur from neglect of this point, and operations performed as primary where too much exhaustion or shock exists; the chloroform, especially if carelessly administered, being the last straw on the camel's back under which the patient sinks.

As to the administration, the author submits (4thly) as the result of much observation in hospitals, that it is desirable the peculiarities of chloroform should be more widely taught and known amongst students and in general practice, as much of the panic in coroners' courts about deaths arises from want of skill in these details. Thus, bad hernia cases strangulated come to hospital ten or a dozen or twenty miles to be operated on, as the author has seen them, but the hernia reduced at once without operation under chloroform, the rural practitioner admitting he was afraid of chloroform. He has heard a melancholy argument often urged in one hospital, that as "our grandmothers" did without chloroform, so ought we; and in another hospital the students are occasionally told chloroform is like the philosopher's stone soon to be forgotten, and not worth serious study or notice. It is unpleasant to dwell on such points, but that in spirit they are too much acted on, and in coroners' courts especially much mischief is the result.

5. In medical practice, in such diseases as epilepsy, puerperal mania, convulsions in children, tetanus, asthma, &c., as also in midwifery practice, where chloroform is a true charity to agonized mothers, it has been and continues to be very unpleasant to meet the objections just noticed. If our predecessors had not chloroform and got on very well, neither had they vaccination nor railways, which we could just as soon ignore as anæsthetics. In tetanus chloroform has not been used as often as it deserves to be. It is to be feared, too, that now quackery is making way as to chloroform where legitimate inquiry fails.

6. Sulphuric ether as an anæsthetic has been carefully studied by the author. It is very valuable as an agent to be *held in reserve* to give alternately with chloroform. It appears to set up a new action in the lungs when the pulse is inclined to stop, and restores the pulse in a very marked manner through the lungs.

In fine, it appears from abundant evidence, and what has been already stated, that if in practice we are to be saved from the undeserved censure of the public, or coroners' juries, or the public press; if in practice we are anxious (as surely every one is anxious and eager) to lessen the number of these melancholy accidents; if the truth, as already hinted, has had many prejudices to do battle with, and has been but slowly arriving as to chloroform;—that we must still keep on a level with the advancing series of physiological and clinical facts.

No doubt chloroform to some is a very trivial thing, any body may apply it hundreds and thousands of times and give its dangers not even a passing thought; any body might likewise, with similar logic, take charge of an express train on a railway, it will go of itself: but in case of danger looming in the distance, it is a different thing to know exactly what to do!

There are two hundred deaths from chloroform recorded already, and perhaps twice or four times two hundred unrecorded, so that in reality it is not such a trivial thing. Some ideas or words, such as "cardiac syncope," "fatty heart," as the causes of death, as also the objections in the very highest places to chloroform in midwifery, or the argument as to our "grandmothers" and the like, all curious and suggestive things in their day,—are now like old coins, to be removed out of circulation as of no intrinsic value.

The utmost contradiction exists in books and schools and journals as to what is best to be done at the operating table in accidents. When have we to fear apnœa? What is the real value of ether? How does apnœa differ from syncope? What kind of inhaler is best? What is this "Faradization" that has proved so perfect in restoring life? A few words of explanation of each the author wishes further to indicate, as well as a short abstract of the case, the more immediate subjects of this communication.

Any irritation of the special nerves distributed to the mucous membrane of one part of the larynx, according to some modern experiments of Schiff, Brown-Séquard, and Rosenthal, at once stops the action of the diaphragm, we have in such a condition some spasm of the glottis and a tetanic state of the respiratory muscles and apnœa. This has been demonstrated by pinching the nerve at its root. It is a nerve or pair of nerves not usually described in books, coming off from the eighth pair above the superior laryngeal, or even irritation of a like kind of the "recurrent" laryngeal, produces sudden closure of the glottis. This spasm, possibly excited by irritation of acid or acrid chloroform, or even a foreign body, such as vomited matters, or loose fibres from a piece of lint or rough napkin used as inhalers getting into a half-narcotized larynx, the author has sometimes suspected as the cause of fatal accident by apnœa. Even in an animal all but dead and restored again to life, for instance, by electricity, as Schiff has demonstrated, there is one point during the resuscitation, if one irritates these nerves, their "inhibitory" action, as it is sometimes called, is brought into play, there is instant spasm again, apnœa, and death. We are here evidently on very dangerous ground, corroborative of which it may also be mentioned that the author has heard Mr. Hilton, who has given such attention to the anatomy of this region, to state, in clinical remarks in the wards of Guy's, that surgical operations about the larynx and neck are peculiarly dangerous from the complexity of the nerves in this situation.

It is not intended by these complex studies of these nerves to frighten surgeons as to the complexity of chloroform administration, which is in reality one of the simplest things in the world. The danger from apnœa also occurs occasionally, when the hospital surgeon is absent, and, as a rule, in trivial surgical accidents and trivial surgical operations.

And first, as to the occurrence of apnœa.

What are the indications afforded already as to when we have to apprehend the occurrence of apnœa? It is very singular, but no less true (it is an observation of Snow's corroborated by further experience), that it is really in perfectly healthy individuals, healthy adult men, and in the most trivial operations, where the constitution has suffered least, that we have to fear the occurrence of deaths from chloroform; for example, in healthy persons who have gone to a dentist to have teeth extracted, strong men who have walked to hospital to have a finger amputated, a catheter passed, a dislocation reduced, or, for example, a delirium tremens patient of bad stamina who has had any of these operations done for him. Accidents almost never occur in great operations.

There may be something faulty in the mode of administration of the chloroform in such cases, in the confusion of students in the out patient's department, or error in the dentist's assistant, who is usually most profoundly innocent of all about anæsthetics and the modes of resuscitation; but a more important element seems to be, that we have here a man in perfect and active life, active circulation in the brain, good reflex tone of his respiratory nerves—a man, in a word, with a large amount of vigorous life, which in a moment is extinguished by the chloroform. He requires, too, a very large quantity of chloroform to dull this sensibility; the man himself, as the author has known, will take, unknown to the surgeon, also a large bottle of porter with gin to keep his courage up.

There are present also, in such a case, perhaps what are most dangerous, nervous exhaustion from anxiety or loss of food, sudden but perhaps intense anxiety about the accident or operation. He has got his finger crushed in machinery perhaps.

It is curious indeed that in other or the great operations of surgery, such as ovariotomy, amputation of the thigh, &c., the "law of tolerance" first noted by Miller is very observable; such patients are reduced and require a smaller quantity of chloroform.

Even as to *sex* there is the greatest possible difference on this point. Females bear pain better than males, and of some five hundred accidents of all kinds the proportion of fatal cases was at one time three to one of men as compared to women, and now it is about two and a-half deaths of men to one woman. No doubt some of this is due to the fact that men are more exposed to surgical accidents; but on the other hand, we have had now some sixty or one hundred thousand cases of midwifery under chloroform, and a series of hundreds of operations on the female organs, such as ovariotomy, vesico-vaginal fistula and the like, to put against this fact as to men, but still the average or proportion of two and a-half to one holds good. The deaths, too, are more frequent about the same age—20 to 40—as children are free, as well as old people, from deaths by chloroform, though used extensively amongst children. In women good respiratory reflex irritability saves them.

These statistics, no doubt, are yet too limited for the elimination of the *one* or great cause of death by chloroform; taken, however, with the previous clinical facts, and the admirable remedy which electricity has proved in hundreds of experiments on animals in restoring life, the statistics point to the direction where the danger lies. The author wishes to avoid hasty generalization, which created errors in the former induction from the facts as to cardiac syncope, heart disease, fatty heart, &c. We do not meet fatty heart at all in a single one of the cases recorded, nor is it likely to occur in young vigorous active men with bounding strong pulse, who are the usual victims of chloroform.

These statistics point the way, and it will be wise to adopt the lesson they teach. No doubt algebraic laws of "variables" would say we should have hundreds of thousands of cases to base an induction upon; but it may be open to doubt if such laws of variables are not rather an obstruction to practical medicine than an assistance.—*Dublin Medical Press, May* 18, 1864, *p.* 516.

121.—ON SCIATICA.

By Dr. Henry Wm. Fuller, Physician to St. George's Hospital.

In one class of cases sciatica is connected with sympathetic irritation, excited by long-continued loading of the bowels, or by the presence of crude irritating secretions in the primæ viæ. In many of these instances the patients have never suffered from gout or rheumatism, and there is an absence of symptoms indicative of hip disease, of renal irritation, or of general nervous irritation, as in the cases last mentioned. The skin is cool, the complexion is seldom pallid as in cases of rheumatism, the urine is usually clear and often pale, there is no pain in, or retraction of the testicle, and no pain in the course of the ureter. Further, the pain is usually felt down the course of both sciatic nerves, instead of being confined to one side, the tongue is furred, and the bowels are costive. In this class of cases the indications for treatment are obvious enough. It behoves us to act fully on the bowels, so as to rid them of all irritating secretions and of the hardened fæces which are often impacted in them. But ordinary purgatives are of little avail for this purpose. Experience proves that colocynth pills and senna draughts and saline aperients will induce free watery evacuations from the primæ viæ, but will fail in removing old accumulations in the bowels, and in relieving the patient's sufferings. The means by which this end must be attained are enemata of a stimulating character, opium followed by full doses of castor oil or croton oil, and the continued use of guaiacum. No purgative is more certain in its operation than guaiacum, and none can be more thoroughly relied upon for affording relief. Combined with sulphur in equal proportions, in the form of powder, it proves a never-failing aperient if given in half drachm doses twice or three times daily; and if administered only in appropriate cases, it proves one of our most trustworthy allies. You have seen me employ it so often in this hospital that it is scarcely necessary to direct your attention to it more particularly, but I will even detain you a few minutes whilst I recall the symptoms of J. S., aged fifty, who was admitted into the Fuller ward on the 5th of May. This man was attacked with sciatica of both sides six months before admission, and although the pain had varied in intensity from time to time, it had never ceased since the commencement of the attack. He described the pain as of a dull aching or gnawing character, slightly aggravated by motion. It did not increase in severity at night, and it had not been accompanied by pain in any other part of the body. He had never suffered from gout or rheumatism, and his general health was good; his aspect was healthy; tongue coated; bowels reported open, though usually costive; urine clear and acid; pulse 76, soft. Here, then, was a case in which all the causes of sciatic pain which we have hitherto considered appeared to be absent. There were no symptoms of local mischief in the spinal cord, or in the sciatic nerves, or of hip disease, or of renal irritation; no evidence of the existence of gout or rheumatism, or venereal taint; no mark of general nervous irritability, requiring sedatives for its subjugation. The patient was not a weakly or a nervous man, and his pulse was steady and quiet. To what, then, was the pain attributable? What indications were there for treatment? One, and one only, could be discovered. His tongue was coated, and his bowels were usually costive. True, they had acted on the morning of his admission, but when acrid or hardened fæces accumulate in the lower bowels, they do not necessarily obstruct the passage so far as to prevent even a daily action of the bowels. Nay, rather they are apt to excite local irritation, and a frequent desire to go to stool, which often leads the patient to imagine that he is suffering from diarrhœa. You have often noted cases in the wards of this hospital in which a spurious diarrhœa, excited and kept up by the cause under discussion, has resisted chalk mixture and various astringents, and has yielded immediately to a dose of castor oil which has brought away a quantity of hardened fæces, or acrid unhealthy secretions. Therefore it was that in the case of J. S., I did not allow the alleged action of the bowels to deter me from following out the plan of treatment I had resolved upon after a careful consideration of the symptoms of the case. If the pain were not excited sympathetically with the presence of acrid irritating matter in the intestines, to what could it be attributable? The closest inquiry had failed to elicit the slightest evidence of the existence of the other causes of sciatica, and if the cause under discussion had not excited the pain, there was no indication for treatment. Accordingly I determined to make trial of remedies which will usually get rid of fæcal accumulations, and thus effect the object I had in view. Whilst allowing the patient a full and generous diet, with a pint of porter daily, I prescribed a powder, to be taken three times a day, consisting of two scruples of guaiacum and two of sulphur. This treatment was commenced on the 5th of May, and was continued until the 11th. At that date the pain was greatly relieved, and the bowels were acting so freely that the powder was repeated only night and morning. From this time he took it usually twice, but sometimes once a day, according to the action of the bowels, until the 19th, when, as he no longer suffered from pain, he left the hospital, and returned to work.

The instances I have hitherto brought before you have been examples of sciatica in which, the cause of the disease having been correctly diagnosed, the treatment was adapted to the exigencies of the case, and relief was speedily obtained. But although I have wished to bring prominently before you the possibility—nay, the probability—of affording speedy relief, if an appropriate method of treatment is employed, and the equal probability that failure will attend your efforts if you do not correctly ascertain the cause of the sciatic pain, and, therefore, cannot determine the class of remedies which are needed, I would caution you against putting a too favourable interpretation on my remarks, and imagining that sciatica ought in all cases to be got rid of within a few days of the commencement of treatment. Some cases there are, as you have already seen, which admit of relief in a very short time, but there are others, and unfortunately a large proportion, in which the patient's health is undermined, and cannot be so speedily restored, even though the nature of the derangement be correctly diagnosed; and others, again, in which it is almost impossible at the outset of the attack to arrive at a correct conclusion respecting the causation of the pain, and in which even if a correct judgment were formed on this point, it would still be impossible to afford speedy relief. Let me instance the case of P. D., aged forty-eight, who was admitted into the Hope ward on the 11th of

December. This man, a "commissionaire" by occupation, had suffered nine months from pain down the course of the right sciatic nerve. It came on gradually, and he attributed it to the effect of exposure to wet and cold. The pain was constant, but was worse at night, and prevented his obtaining quiet rest. A variety of external applications had been made use of prior to his admission to the hospital, but the pain had steadily increased in severity. On admission his aspect was healthy; his skin natural; pulse 84, of fair strength; tongue rather coated; bowels reported regular; urine scanty, high-coloured, and turbid; appetite good. He had never suffered from gout or rheumatism, or renal irritation, but he was extremely nervous. Judging from his history, and from the condition of his tongue and urine, I was led to regard his symptoms as attributable to rheumatic irritation of the nerve, aggravated by his nervous temperament. Acting upon this view, I prescribed a subcutaneous injection of half a grain of morphia at night, and a powder, three times a day, containing guaiacum, sulphur, and carbonate of soda, a scruple of each. This treatment was pursued until the 17th, when, as no relief had been obtained, he was ordered five grains of iodide of potassium, half a drachm of bicarbonate of potash, and an ounce and a half of nitre draught, every six hours. On the 21st his pain remained undiminished, and he complained of feeling weaker; and as his tongue had cleaned, and his urine had become clear, a drachm of the sesquioxide of iron was administered three times daily, and the biniodide of mercury ointment was ordered to be rubbed in along the course of the nerve. So he went on until the 2nd of January, when he complained of so much pain in the hip that I was again induced to make a careful examination of the joint. I then discovered considerable tumidity and general enlargement over the joint, which was painful on pressure, and evidently much distended with fluid. He could move his leg carefully without pain, but could not bear the head of the femur to be pressed upwards against its socket. Under these circumstances I applied a blister to the hip, and had the blister surface dressed with mercurial ointment; at the same time I gave him a morphia draught at night, and ordered him to take three times a day a nitre draught, containing six grains of iodide of potassium, and two drachms of the solution of bichloride of mercury, forming a soluble biniodide of mercury. Notwithstanding this treatment the pain became more constant and severe, and on the 15th, as the mischief appeared to be purely local, and the occurrence of ulceration of the cartilages more than probable, he was transferred to the care of the surgeons.

Now I would have you remark in reference to this case—

1st, that the nature of the disease was overlooked, not only by those who had charge of the man prior to his admission to the hospital, but also by myself on his admission into the hospital, and that it was not until after the lapse of three weeks, when the enlargement of the hip attracted my notice that I became aware of the serious mischief I had to combat. The result—the inevitable result of this non-appreciation of the nature and extent of the mischief—was want of success in affording relief, and a steady progression of the disease. Probably the man was correct in his assertion, that the malady was originally of rheumatic origin, for he was healthy in appearance, and had not experienced any local injury of a nature to set up disease in the hip-joint. It is quite conceivable, therefore, that if, when he was first attacked by the pain, its rheumatic nature had been recognised, and he had been subjected to treatment calculated to get rid of the rheumatic tendency, instead of being merely treated by embrocations, lotions, and other local applications, which could have no influence on the cause of the disease, and little effect in subduing its local consequences, he might have escaped the injury to the joint, which, doubtless, was commencing at the time of his admission into the hospital. In like manner, if the true extent and character of the local mischief had been discovered when he first presented himself to my notice, and those measures had been at that time adopted to which I was ultimately obliged to have recourse, he might even then have been saved much unnecessary suffering. Rely upon it, gentlemen, the success of treatment depends upon its adaptation to the requirements of the case, and that in sciatica the non-success of any particular plan which may be adopted is in itself conclusive evidence that the remedies are not in keeping with the nature of the case. If the pain is referable to any removable cause, and the remedies are calculated to subdue or get rid of it, relief will be experienced in the course of a few days, and the patient will thenceforward proceed steadily to recovery; and if you fail in any instance to afford relief in ten days or a fortnight, you will always do well to mistrust your diagnosis, and make a fresh examination of your patient. In the case of P. D., I deferred doing so until after the lapse of three weeks, under the belief that my diagnosis was correct, and that a change of remedies might prove beneficial. The result shows how wrong I was in transgressing the rule which I have laid down for your guidance; and if you require any further incentive to induce you to follow my precept, rather than the example which I set you in this case, I may assure you that I have scarcely ever been called to see a case in which this rule had been departed from without feeling how much better the patient would have fared had his medical attendant been less confident in his diagnosis, and more disposed to pay heed to the teachings of nature as evidenced by the result of treatment.—*Lancet, April 23, 1864, p. 459.*

122.—ACID AND SUGAR IN SPIRITUOUS LIQUORS.

From Editor's Appendix to "The Chemistry of Wine," by G. J. Mulder. Edited by Dr. H. Bence Jones, F.R.S.

Acid.—"Proceeding from the least acid wine to the most acid, we have Sherry, Port, Champagne, Claret, Madeira, Burgundy, Rhine Wine, Moselle. The least acid fluids examined, were Geneva and Whisky; then Rum, Brandy, Ale, Porter, Stout; the wines were all more acid than the malt-liquors. The nature of the acid was not absolutely determined, but a volatile acid distils over from wine which is not acetic; and the action of polarised light shows that tartaric acid is seldom present. Hence the fixed acid is most probably racemic, and, perhaps, malic acid."

Sugar.—"I found no Sherry, Port, Maderia, or Champagne that did not contain more or less uncrystallizable Sugar (two samples of Sherry excepted, which were free from Sugar). I met with no Claret, Burgundy, Rhine, or Moselle wine (excepting only one sample of Sauterne), which was not free from every kind of sugar. Usually, spirits contain no Sugar; but one specimen of genuine French Brandy had some cane-sugar added to it. All kinds of Ale, Porter, and Stout, contain much glucose (grape-sugar). Hard Cider, I found also to be perfectly free from Sugar. Sweet Cider contains uncrystallizable Sugar."

"The fluids examined may be arranged in the following order, commencing with those which contain no Sugar, and ending with the most Saccharine. Geneva, Rum, Whisky, Claret, Burgundy, Rhine, Moselle. These contain no Sugar—Brandy, Sherry, Madeira, Champagne, Port, Cider, Porter, Stout, Malmsey, Ale, Tokay, Samos, Paxarette, Cyprus."—*Dr. Dobell's Manual of Diet and Regimen, p.* 31.

123.—ON THE RADICAL CURE OF INGUINAL RUPTURE.

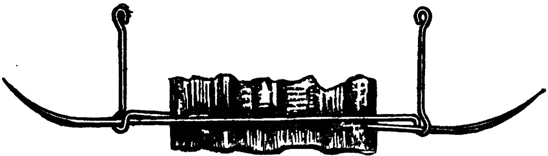
By John Wood, Esq., Assistant Surgeon to King's College Hospital.

Operation by Pins.—This operation I use in cases of congenital hernia, and those of small size in lads and young men. In such cases, when the hernial canal is narrow, its sides yielding, and easily and completely approximated, there is no need of any intervening substance to fill up the hernial gap. The tissues also are thin and comparatively unaltered, and permit of an accurate examination, by the finger, through them when invaginated into the canal,

The apparatus used in this operation is very simple, and no incision through the skin is required.

The pins I employ are of various sizes—from three to five inches long, hardened at the point, but made in the shaft so that they will bend a little rather than break off short.

FIG. 1.



They are spear-pointed, curved boldly near the point like a suture needle, flattened on the concavity, with slightly cutting edges, and with a rounded shaft. At three or four inches from the point the shaft is bent into a right angle and twisted into a loop large enough to receive the point of a fellow needle. The projecting end is about an inch long, terminating in another loop for the purpose of being held by the dressings. See Fig. 1. When applied, the pin should be held by the middle of the shaft between the thumb and long finger, the end of the forefinger being placed upon the bent extremity. They are applied in opposite directions through the structures intended to become adherent, and are pinned through and through like an ordinary toilet pin. Each point is then passed through the angle loop of the fellow pin, and the ends then rotated in opposite directions, so as to twist up the confined tissues, and fixed by straps of plaster and pads.

The patient being laid on his back with the knees and shoulders raised, the scrotum is first invaginated into the canal, and the relative positions of the cord, the conjoined tendon, and Poupart's ligament carefully noted. In small cases this can be most conveniently done with the little finger. The finger should be carried as far up as possible, so as to fill in front of it the lower border of the internal oblique muscle, and to the inner side of it the conjoined tendon. The skin of the groin being then drawn directly inwards by an assistant, the needle is carried with its concavity outwards and downwards through the tissues directly upon the inside of the nail of the invaginating finger (see Fig. 2). The point is then turned downwards towards the pubis, and pushed steadily after the invaginating finger as it is slowly withdrawn, protecting the needle on the outer side.

As the needle passes over the pubis it is made to take up the fibres of the internal pillar attached to that bone, grazing the bone as it does so. The needle and finger must be kept fairly together, the two last fingers of the hand resting firmly upon the groin over the deep ring. This gives steadiness to the hand, and, at the same time, by its pressure, keeps the bowel out of the canal. The point of the pin is then to be cut off with a pair of pliers. The second pin is then taken, and its point, with the concavity directed forwards, passed into the aperture through which the first emerges in the scrotum. The invaginating finger is then placed below it, and passed up into the canal, carrying the pin before it till it touches the middle of the back

FIG. 2.

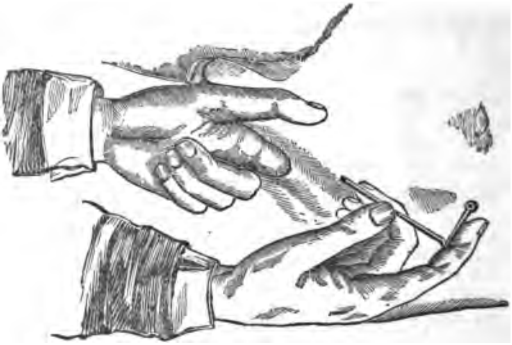
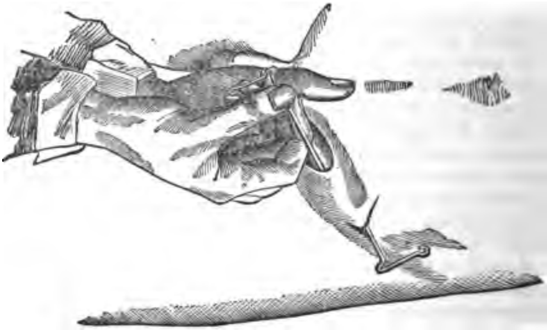
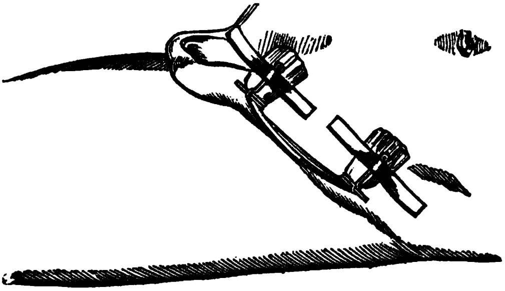


FIG. 3.



surface of Poupart's ligament. Through this the pin is then pushed upwards and outwards till it can be seen to raise the skin (see Fig. 3). This is then drawn inwards by the assistant until the point of the pin can be pushed through at the same place at which the first pin entered. The pins are then locked, after which the point of the second pin is to be cut off conveniently close, and one of the bent ends of the pins twisted over the opposite side and held down by a pad of lint and a strap of adhesive plaster. A strip of oiled lint is then twisted round the pins where they lock, so as to protect the skin from their pressure somewhat (Fig. 4). The whole is then retained by a pad and spica bandage.

FIG. 4.



In this operation the conjoined tendon and inner pillar are transfixed by the first pin, which thus traverse the inner and hinder part of the hernial canal and sac, while the outer pillar is fixed at Poupart's ligament by the lower pin. The whole of these parts, with the hernial sac, are twisted together by the revolution of one pin round the axis of the other, so that the hinder wall is drawn forcibly forwards and outwards, and the front walls backwards and inwards till the canal is firmly closed. The cord lies behind the pins, closely embraced by the twisted tissues. Adhesion takes place between the parts in their new position, following up closely (if the case does well) the slow ulceration in the course of the pins. These are kept in a week or ten days, according to the amount of fibrinous effusion which takes place in the canal. About that time a thick firm mass of deposit occupies the course of the pins, excluding the bowels, filling up the sac, and uniting all the twisted tissues into a solid mass. When the pins are withdrawn, simple water dressing or a poultice may be applied. As the wound heals, a stout pad should be applied over the dressings, and held in its place by a bandage firmly applied. When the openings have sufficiently healed, a horseshoe-pad truss should be applied, with a bit of cotton wool or lint between it and the skin to protect the tender skin. Until this time the patient should be kept in the recumbent position. After the truss has been applied, he may be suffered to get up.

If, in the course of the treatment, the testicle becomes much swollen, all bandages and pressure should be removed, and cold water or evaporating lotion applied. Some degree of swelling of the testicle is beneficial, rather than otherwise, as showing a sufficiently close embrace of the cord, and, consequently, obliteration of the canal. The pins usually are withdrawn with great ease and very little pain. The patients ordinarily suffer so little, that food is taken in the usual way a day or two after the operation. I seldom find it necessary to employ a stricter regimen than milk diet for the first few days. As the wound heals, an allowance of wine or beer may be permitted, and the general health of the patient supported as much as possible, to ensure a more vigorous reparative formation in the parts.

In some cases I have applied the pins transversely to the canal, especially where, as in cases of a large size and direct kind, the rupture shows a tendency to emerge behind the cord, lifting it forwards and pushing it outwards. In some of these very large cases it has been necessary to repeat the operation in order to meet this tendency. Wire has also been applied transversely under these circumstances. It is important in such cases to make the deeper pins or wire touch or graze the upper border of the pubis, over which the rupture emerges. By establishing a bridge of adhesion there, this tendency is better counteracted.

In the case of a child, aged three years, now in the hospital, a very large rupture, reaching half-way down the thigh, and totally unmanageable by any kind of truss, has been very successfully treated. No impulse whatever is now felt on crying, and the large sac can be felt doubled up in the scrotum, and forming a hard tumour, connected above with the pillars of the ring and canal. The child's health and appearance have positively improved during the time that he has been under treatment. I have found it very advantageous in this and other large cases to keep the child's head and shoulders laid a little lower than his body and legs. This diminishes much the force with which the rupture presses from above upon the newly-formed adhesions. In these large cases the ring truss pad, described in a former paper, will be found to give a more direct and efficient support to the adhesions. At the same time it does not press upon the structures which fill up the hernial gap in the abdominal walls. The sides of the inguinal canal and the pillars of the superficial ring, already closed up by the operation, are kept in position by the pressure of the ring or horse-shoe truss-pad. The first complete closure and obliteration is effected by the operation, and the truss, by supporting properly the newly-formed tissues, during some months after the operation, completes the cure.

I have kept cases treated upon this plan in view for two or three years, the rupture remaining perfectly cured up to the present time, without any truss being worn after the first six or eight months. The growth and development of young subjects assist powerfully in rendering the resistance to reprotrusion more efficient as time elapses, during which the bowel is effectively excluded from the canal. The very large cases of the two brothers Dunn (47 and 48 in my work on Rupture), operated on in June, 1862, remain, according to a letter from the father received a short time ago, perfectly cured up to the present time.

I may here mention that, as I am anxious to obtain, as far as possible, correct statistics of my cases of radical cure, I should be glad to receive information that may have been directly verified by any of my readers as to the failure of any of my cases, specifying the name and address of patient, date of operation, and whether one of my trusses had been worn.

It is the interest both of the profession and of the public to have the utmost light thrown upon the results of an operation for the cure of a deformity which has baffled the efforts of surgery for the last thousand years or upwards. I should be also very glad to know the results that have been obtained by any of my professional brethren, both in this country and abroad, who have given these methods of operating a fair trial. I would emphasise a *fair* trial, because many operations have been done based upon the principles, wholly or partly, which I originally laid down as necessary for the cure of rupture, but altered by modifications according to the fancy of the operator, many of which (from the accounts I have received) have influenced the results of the operation very materially.—*Medical Times and Gazette, May* 7, 1863, *p.* 503.

124.—ON URÆMIA.

By Dr. George Harley, Assistant Physician to University College Hospital.

Uræmic poisoning is a subject of such grave importance to the Medical man that I must say a few words regarding it. There can be no doubt that urea is a powerful irritant poison.

When injected into the veins of animals it rapidly induces fatal convulsions. It is not necessary, as some have imagined, that the urea should be decomposed in the blood in order to produce its toxic effects. On the contrary, I believe that urea is a far more dangerous poison than the substance into which it is decomposed, namely, the carbonate of ammonia. Both are, however, poisonous, and they frequently manifest their action together, for although we may, perhaps, have uræmic without ammoniacal poisoning, we cannot have the latter, as the result of disease, without its being in some measure associated with the former.

True uræmia depends on the arrested elimination of the poisonous material by the kidneys; true ammonæmia on the re-absorption into the circulation of the decomposed secreted product.

Uræmia may occur in the course of any disease in which suppression of the renal secretion takes place. It is therefore superfluous to specify the diseases in which it may occur by name.

Carbonate of ammonia poisoning occurs in its least complicated form in those cases where the urea, although secreted, is retained in the urinary passages until it is decomposed, and the products of its decomposition are reabsorbed into the blood. Just as was formerly said the normal ingredients are reabsorbed from the bladder, when from any cause the urine is retained for some length of time in that viscus. The conditions under which it most commonly occurs are therefore:—

1st. Sacculated kidneys.

2nd. Dilatation of the ureter and pelvis of the kidneys.

3rd. Renal abscess.

4th. Paralysis of bladder.

5th. Enlarged prostate with retention.

6th. Stricture with retention; or, indeed, retention from any cause whatever,—all that is required to induce ammonæmia being the retention of the urine in the body sufficiently long to allow of the decomposition of its urea. It is quite surprising, indeed, in how very short a time retained urine may become ammoniacal.

In Mr. Marshall's case of exfoliation of the bladder, which has during the last year given rise to so much discussion, the urine contained such an abundance of carbonate of ammonia that it effervesced exactly like a seidlitz powder when I added to it a few drops of acid.

The symptoms which distinguish ammonæmia from simple uræmic poisoning have been recently carefully pointed out by Jacksch (1860).

In ammonæmia the urine is ammoniacal when passed. The breath and perspiration are ammoniacal. The mucous membrane of the mouth is dry and shining. The complexion is sallow, and there is increasing emaciation. No dropsical symptoms are present. Convulsions are rare, but intermittent ague is frequent. Moreover, although in the acute form of the disease both vomiting and diarrhœa may occur, in chronic cases, which are much more common, these symptoms are always absent. Death is in general preceded by coma, varying from a few hours to several days in duration.

It is thus seen that the symptoms, as well as the pathology of ammonæmia, present striking features of difference to those of pure uræmia. The most characteristic of the latter being the vomiting, and diarrhœa, the convulsions, and coma. The treatment of the two cases is in several particulars equally different.

In the first place, ammonæmia arising, as it generally does, from directly remediable causes, is much more frequently under our control than uræmia. For example, when the ammonæmia is due to the simple retention of urine in the bladder repeated catheterisation is usually followed by a speedy cure. In no case is uræmia within the reach of instrumental interference. Its most remediable form is when it arises from some temporary incapacity of the kidneys to perform their office; as in suppression of the urine after scarlet fever, or in idiopathic nephritis. Here we can do good by relieving the congestion of the kidneys by cupping or leeching the loins; and when the patient is too weak for such active measures, by the application of dry cupping-glasses, poultices, and the free use of the air-bath. In most other particulars, the two diseases are to be treated alike; and when neither offers us a chance of permanent cure we can in general alleviate the sufferings, and prolong the life of the patient by reducining the formation, as well as by hastening the elimination of the urea. This may be accomplished by a proper selection of food and medicine.

The remedies which enable us to control the formation, and hasten the elimination, of urea are the following:—Whenever there is an excessive tissue waste, as indicated by an augmented excretion of urea, without any very apparent cause, the citrate of quinine and iron appears to be a most useful remedy. Sigmund has observed that digitalis diminishes the elimination of urea; and Kletzinsky, that benzoic acid has a similar effect, but in a much greater degree. The last-named observer states that the daily excretion of urea may actually be reduced to 2·5 grm. (38·75 grains), by benzoic acid, in the short space of twenty-four hours. The acetate and phosphate of soda, as well as colchicum, act in a similar manner (Parkes). On the other hand, we have several remedies which increase it. Thus, Sigmund found that cubebs and cantharides have this effect; and Parkes has noted that atropine augments its elimination. If we wish to diminish the urea by means of the diet, all that is necessary is to administer arrowroot, sago, tapioca, and other such starchy foods, well sweetened with sugar; and when a more nourishing food than this is demanded, cream, cod-liver oil, or any other fatty matters that may be considered suitable to the peculiar case, will have an equally good effect in reducing the amount of urea. On the other hand, when it is deemed advisable to increase it, animal soups, eggs, milk, jellies, and other nitrogenised matters, together with a fair portion of common salt, will answer the purpose. To these may be added coffee, but without sugar.

In concluding my remarks on urea, I have to remind you how useful a knowledge of its daily excretion is in prognosis. Thus, it will have been gleaned from what has already been said regarding the amount of urea in the urine during the course of disease, that if, in cases of fever, such as typhoid and typhus, or of the exanthemata, such as small-pox and measles, or of inflammatory affections, such as pneumonia and meningitis, a decrease in the daily elimination of urea is observed, it may be regarded as a most favourable sign for the prognosis; for no sooner does a change for the better take place in these affections, than an immediate diminution in the amount of the urea is observable. During convalescence, the quantity is frequently below the normal standard. Whereas, in cases tending to a fatal termination, even in spite of the true febrile symptoms having passed away, the daily excretion of urea still remains high. Hence this sign may be a valuable and truthful guide when all others fail. If, on the other hand, in those diseases in which the excretion of urea is known to be abnormally small, such, for instance, as paralysis, cholera, or the different forms of albuminuria, an increase in its amount during the course of the case is noticed, it is an equally favourable sign for the prognosis; while on the contrary, any further reduction in the amount of the eliminated urea cannot be otherwise regarded than a most untoward event, as, even when the other symptoms have improved, it is an almost infallible index of approaching danger.—*Medical Times and Gazette, April* 30, 1864, *p.* 474.

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